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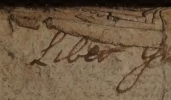

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TRACTS:

CONTAINING

I. SUSPICIONS about some *Hidden Qualities* of the *AIR*; with an *Appendix* touching *CELESTIAL MAGNETS*, and some other Particulars.

II. ANIMADVERSIONS upon Mr. *Hobbes*'s *PROBLEMATA De VACUO.*

III. A DISCOURSE of the CAUSE of *Attraction* by *SUCTION.*

By the Honourable

ROBERT BOYLE, Esq;

Fellow of the ROYAL SOCIETY.

LONDON,

Printed by *W.G.* and are to be Sold by *M.Pitt*, at the *Angel* against the Little North Door of St. *Paul*'s Church. 1674.

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SOME ADDITIONAL

EXPERIMENTS

Relating to the

SUSPICIONS

ABOUT THE

HIDDEN QUALITIES

of the AIR.

By the Honourable

ROBERT BOYLE,

Fellow of the *Royal Society*.

LONDON,

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SOME ADDITIONAL *EXPERIMENTS* Relating to the SUSPICIONS ABOUT THE *Hidden Qualities of the AIR.*

THE ESSAY about *Suspicions of some hidden Qualities of the Air*, having been detain'd somewhat long at the Press, that it might come abroad accompanied with the other Tracts design'd to attend it, whilst I was

rumaging among several Papers to look for some other things, I met now and then with an Experiment or Observation, that seem'd to relate to some of the things deliver'd in that *Tract*; and though they be in themselves of no great moment, I am content to annex them to the rest, because, as in that company they may signifie somewhat, so I am unwilling that any matter of fact, relating to such a Subject, should perish to save the labour of transcribing.

EXPER. I.

Having occasion to dulcifie some *Calx* of *Dantzig-Vitriol*, from which the Oil had been a good while before distill'd; water was put upon two large portions of it, that the liquor might be impregnated with the Vitriolate particles remaining in the *Calx*; the water put upon one of these portions was, soon after it was sufficiently

ciently impregnated, filtrated and gently abſtracted, by which means it afforded many drams of a kind of Salt of Vitriol that ſeem'd to differ very little from the Vitriol that had been calcin'd: But the water that was put upon the other portion of calcin'd Vitriol, was in a wide-mouth'd veſſel left in the Air for a month or ſix weeks; after which time, when it came to be abſtracted after the manner formerly recited, it afforded many drams of a Salt that did not then, nor long after, look at all like common Vitriol, or like the other, but ſhot white almoſt like Salt-petre, or ſome other untincted Salt. Whether this Experiment will conſtantly ſucceed, and at other Seaſons of the Year than that 'twas made in, which was Summer, I had not the opportunity to make a full trial; though I endeavour'd it. But that the Air may have a great ſtroke in varying the Salts obtainable from calcin'd Vitriol, ſeem'd the more probable, be-

cause we had some Colcothar that had lain many months, if not some years, in the Air, but in a place shelter'd from the Rain; and having caus'd a *lixivium* to be made of it, to try what sort or plenty of Saline particles it would yield, we found, when the superfluous moisture was exhaled, that they began to shoot into Salt far more white than Vitriol, and very differing from it in its figure and way of Concretion.

EXPER. II.

We took Colcothar of Venereal Vitriol carefully dulcified, and leaving it in my Study in the Month of *January* and *February*, by weighing it carefully before an ounce of it was expos'd to the Air, and after it had continued there some weeks, we found it to have increas'd in weight four grains and about a quarter, besides

This was made at Oxford.

sides some little dust that stuck to the Glass.

This slight Experiment is here mention'd, that, being compar'd with the next ensuing Trial, it may appear, that the difference of Airs, Seasons, *Calces* of Vitriol, or other Circumstances, may produce a notable disparity in the Increment of weight, the exposed Bodies gain in the Air.

EXPER. III.

We put eight ounces of Outlandish Vitriol, calcin'd to a deep redness, into a somewhat broad and flat Metalline vessel, and set it by upon a shelf, in a Study that was seldom frequented; and at the same time, that we might observe what increment would be gain'd by exposing to the Air a larger *superficies* of the powder in reference to the bulk, we put into another Metalline vessel, smaller than the other, only two ounces of Col-

cothar, and ſet it on the ſame ſhelf with the other, this was done at the Vernal Equinox, (the Twelfth of *March*;) on the twenty fifth of *June* we weigh'd theſe powders again, and found the eight ounces to have gained one dram and ſeventeen grains; but the two ounces had acquired the ſame weight within a grain: Then putting them back into their former veſſels, we left them in the ſame place as formerly, 'till the twenty fourth of *Auguſt*; when we found cauſe to ſuppoſe, that the greater parcel of Colcothar had met with ſome miſchance, either by Mice or otherwiſe; but the leſſer parcel weigh'd Twenty ſix grains heavier than it did in *June*, amounting now to two ounces, one dram, forty two grains, having increaſed, in leſs than ſix months, above an hundred grains, and conſequently above a tenth part of its firſt weight.

No Trial was made to diſcover what this acquir'd Subſtance may be,

be, that we might not disturb the intended prosecution of the Experiment.

EXPER. IV.

Because in most of the Experiments of Substances expos'd to be impregnated by the Air, or detain its Saline or other exotic particles, we employed Bodies prepar'd and much alter'd by the previous operation of the Fire; we thought fit to make some Trials with Bodies unchanged by the Fire, and to this purpose we took a Marchasite, which was partly of a shining and partly of a darkish colour, and which seem'd well disposed to afford Vitriol; of this we took several smaller Lumps, that amounted to two ounces; these were kept in a room, where they were freely accessible to the Air, which, by reason that the House, that was seated in the Country, stood high,

was

was esteemed to be very pure. After the Marchasites had been kept in this room somewhat less than seven weeks, we weigh'd them again in the same Ballance, and found the two ounces to have gained above twelve grains in weight.

EXPER. V.

The Experiment us'd at the latter end of our Paper, about *Celestial* and *Aerial Magnets*, seeming to some *Virtuosi* very strange, and the way that I employ'd in making that Liquor, that turns green in the Air, being somewhat troublesome, I remember I thought fit to try upon the same ground a way of producing the same Phænomenon more easie and more expeditious. And though perhaps this way will not succeed so constantly, nor always so well as the other, yet for its easiness and cheapness it will not probably be unwelcome

to

to those that are desirous to see the odd Phænomenon.

We took then, more than once, filings of clean crude Copper, and having put on them a convenient quantity of good Spirit of Salt, we suffer'd the *Menstruum* in Heat (which need not be very great) to work upon the Metal, which it usually does slowly, and not like *Aqua-fortis*: When the Liquor had by this operation acquir'd a thick and muddy colour, we decanted it into a clean Glass with a wide mouth, which being left for a competent time in the open Air, the expos'd Liquor came to be of a fair green, though it did not appear that any thing was precipitated at the bottom, to make it clear.

EX-

EXPER. VI.

Perhaps it may not be impertinent to add, that I once or twice obſerv'd the fumes of a ſharp Liquor to work more quickly or manifeſtly on a certain Metal ſuſtained in the Air, than did the *Menſtruum* it ſelf that emitted thoſe fumes on thoſe parts of the Metal that it cover'd: And this brings into my mind, that, asking divers Queſtions of a Chymiſt that had been in *Hungary* and other parts, purpoſely to ſee the Mines; he anſwer'd me, among other things, that, as to the Ladders and other wooden work imployed in one or more of the deep *Hungarian* Mines, thoſe that were in the upper part of the Groves any thing near the external Air, would by the fretting Exhalations be render'd unſerviceable, in not many months, whereas thoſe Ladders and pieces of Timber, *&c.* that were imployed in the lower part of

of the Mine, would hold good for two or three times as long.

EXPER. VII.

We took about the bigneſs of a Nutmeg of a certain ſoft but conſiſtent Body, that we had caus'd to be Chymically prepared, and which in the free Air would continually emit a thick ſmoak: This being put into a Vial, and placed in a middle ſized Receiver in our Engin, continued for ſome time to afford manifeſt fumes, whilſt the exhauſtion was making; 'till at length, the Air having been more and more pump'd out, the viſible aſcenſion of fumes out of the Vial quite ceas'd, and the matter having remain'd ſome time in this ſtate, the ſmoaking ſubſtance was ſo alter'd, that it would not emit fumes, not only when the Air was let into the Receiver, but not in a pretty while after the Vial was taken out

out of it, 'till it had been removed to the window, where the Wind blowing-in fresh and fresh Air, it began to smoak as formerly.

The other *Phænomena* of this Experiment belong not to this place; but there are two, which will not be impertinent here, and the latter of them may deserve a serious Reflection.

The *first* of them was, that the Substance hitherto mention'd had been kept in a large Glass, whereinto it had been distill'd at least five or six weeks, and yet would smoak very plentifully upon the contact of the Air, and be kept from smoaking, though the Chymical Receiver were stopp'd but with a piece of paper.

The *second* was, that, when the Vial was put unstopp'd in the Receiver, and the Receiver close luted on, though no exhaustion were made, yet the white fumes did very quickly cease to ascend into the Receiver, as if this Smoak participated of

of the nature of Flame, and presently glutted the Air, or otherwise made it unfit (and yet without diminution of its gravity) to raise the Body that should ascend.

FINIS.

FINIS.

SUSPICIONS ABOUT Some *Hidden* QUALITIES in the *AIR.*

BEſides the four firſt Qualities of the Air, (Heat, Cold, Dryneſs and Moiſture) that are known even to the Vulgar; and thoſe more unobvious, that Philoſophers and Chymiſts have diſcovered, ſuch as Gravity, Springineſs, the power of Refracting the beams of Light; *&c.* I have often ſuſpected, that there may be in the Air ſome yet more latent Qualities or Powers differing enough from all theſe, and principally due to the Subſtantial Parts or Ingredients, whereof it conſiſts.

ſiſts. And to this conjecture I have been led, partly (though not only or perhaps chiefly) by conſidering the Conſtitution of that Air we live and breath in, which, to avoid ambiguities, I elſewhere call *Atmoſpherical* Air. For this is not, as many imagine, a Simple and Elementary Body, but a confus'd Aggregate of Effluviums from ſuch differing Bodies, that, though they all agree in conſtituting, by their minuteneſs and various motions, one great maſs of Fluid matter, yet perhaps there is ſcarce a more heterogeneous Body in the world.

And as by Air I underſtand not (as the *Peripateticks* are wont to do) a meer Elementary Body; ſo, when I ſpeak of the Qualities of the Air, I would not be thought to mean ſuch naked and abſtracted Beings (as the *Schools* often tell us of,) but ſuch as they call Qualities *in concreto*, namely Corpuſcles indued with Qualities, or capable of producing them in the Subjects they invade and abound in.

I have elsewhere shewn it to be highly probable, that, besides those vapours and exhalations which by the Heat of the Sun are elevated into the Air, and there afford matter to some Meteors, as Clouds, Rain, Parhelions and Rainbows, there are, at least at some times, and in some places, store of Effluviums emitted from the Subterraneal parts of the Terrestrial Globe; and 'tis no less probable, (from what I have there and elsewhere deliver'd,) that in the Subterraneal Regions there are many Bodies, some fluid and some consistent, which, though of an operative nature, and like upon occasion to emit steams, seldom or never appear upon the surface of the Earth, so that many of them have not so much as names assigned them even by the Mineralists. Now among this multitude and variety of Bodies, that lye buried out of our sight, who can tell but that there may be some, if not many, of a nature very differing from those we are hi-

In a Paper about Subterraneal Steams.

therto familiarly acquainted with; and that, as divers wonderful and peculiar operations of the *Loadſtone*, (though a Mineral many Ages ago famous among Philoſophers and Phyſitians,) were not diſcovered 'till of later Ages, wherein its nobler Virtues have been diſcloſed; ſo there may be other Subterraneous Bodies, that are indowed with conſiderable powers, which to us are yet unknown, and would, if they were known, be found very differing from thoſe of the *Foſſiles* we are wont to deal with:

I alſo further conſider, that, (as I have elſewhere endeavoured to make it probable) the Sun and Planets (to ſay nothing of the Fixt Stars) may have influences here below diſtinct from their Heat and Light. On which Suppoſition it ſeems not abſurd to me to ſuſpect, that the Subtil, but Corporeal, Emanations even of theſe Bodies may (ſometimes at leaſt) reach to our Air, and mingle with thoſe of our Globe in that great receptacle or rendevouz of Celeſtial and Terreſtrial

restrial Effluviums, the *Atmosphere*. And if this suspition be not groundless, the very small knowledge we have of the structure and constitution of Globes so many thousands or hundred of thousands of miles remote from us, and the great ignorance we must be in of the nature of the particular Bodies that may be presum'd to be contain'd in those Globes, (as Minerals and other Bodies are in the Earth,) which in many things appear of kin to those that we inhabit, (as with excellent Telescopes I have often with attention and pleasure observed, particularly in the Moon,) this great imperfection, I say, of our knowledge may keep it from being unreasonable to imagine, that some, if not many, of those Bodies and their effluxions may be of a nature quite differing from those we take notice of here about us, and consequently may operate after a very differing and peculiar manner.

And though the chief of the Heteroclite Effluviums, that indow the

Air with hidden Qualities, may probably proceed from beneath the surface of the Earth, and from the Celestial Bodies; yet I would not deny but that, especially at some times and in some places, the Air may derive multitudes of efficacious particles from its own operations, acting as a fluid Substance upon that vast number and variety of Bodies that are immediately expos'd to it. For, though by reason of its great thinness, and of its being in its usual state devoid both of taste and smell, it seems wholly unfit to be a *Menstruum*; yet I am not sure but it may have a dissolving, or at least a consuming, power on many Bodies, especially such as are peculiarly dispos'd to admit its operations.

For I consider, that the Air has a great advantage by the vast Quantity of it, that may come to work in proportion to the Bodies that are expos'd to it: And I have long thought, that, in divers cases, the Quantity of a *Menstruum* may much more considerably

rably compensate its want of strength, than Chymists are commonly aware of, (as there may be occasion elsewhere to exemplifie.) And there are liquors, which pass for insipid, (and are therefore thought to be altogether unfit to be Solvents,) which, though they have their active parts too thinly dispersed to be able presently to make sensible Impressions upon our Organs of Tasting, yet are not quite destitute of Corpuscles fit to act as a Solvent; especially if they have time enough to make with the other parts of the Fluid such numerous and various motions, as must bring, now some of them, and then others, to hit against the Body expos'd to them. Which may be illustrated by the Rust like to Verdigrease, which we have observ'd in Copper that has been long expos'd to the Air, whose saline particles, little by little, do in tract of time fasten themselves in such numbers to the surface of the Metal as to corrode it, and produce that efflorescence colour'd like Verdi-

greaſe, which you know is a factitious Body, wont to be made of the ſame Metal, corroded by the ſharp Corpuſcles of Vineger, or of the Husks of Grapes: Beſides, that by the power, which *Mercury* has to diſſolve Gold and Silver, it appears, that it is not always neceſſary for the making a Fluid fit to be a Diſſolvent, that it ſhould affect the Taſt. And as to thoſe Bodies, on which the *Aerial Menſtruum* can, though but ſlowly, work, the greateſt quantity of it may bring it this advantage, that, whereas even the ſtrongeſt *Menſtruums*, if they bear no great proportion in bulk to the Bodies they are to work on, are eaſily glutted, and being unable to take up any more, are fain to leave the reſt of the Body undiſſolved, our *Aerial Menſtruum* bears ſo vaſt a proportion to the Bodies expos'd to it, that when one portion of it has impregnated it ſelf as much as 'tis able, there may ſtill come freſh and freſh to work further on the remaining part of the expos'd Body.

Beſides

Besides the Saline and Sulphureous particles, that, at least in some places, may (as I have elsewhere shewn) impregnate the Air, and give it a greater affinity to Chymical *Menstruums* more strictly so called; I am not averse from thinking, that the Air, meerly as a *fluid* Body, that consists of Corpuscles of differing sizes and solidities restlesly and very variously moved, may upon the account of these Corpuscles be still resolving, or preying upon, the particles of the Bodies that are expos'd to their action. For, many of those Aerial Corpuscles, some hitting and some rubbing themselves every minute against those particles of expos'd Bodies that chance to lye in their way, may well, by those numerous occursions and affrictions, strike off and carry along with them now some and then others of those particles; as you see it happens in water, which, as soft and fluid as it is, wears out such hard and solid Bodies as Stones themselves, if it often enough meet them in its passage,

passage, according to the known saying,

Gutta cavat lapidem non vi, sed sæpe cadendo.

And though the Aerial Corpuscles be very minute, and the Bodies expos'd to them oftentimes large and seemingly solid; yet this needs not make you reject our supposition, because 'tis not upon the whole Body at once, that, according to us, the Aerial Corpuscles endeavour to work, but upon the Superficial particles, which may often be more minute than those Corpuscles; as you will the more easily believe, if you *first* observe with a good Microscope, how many extant particles may be met with on the surface of Bodies, that to the naked Eye seem very smooth, and even of those that are polish'd by Art with Tripoly or Puttee; and *then* consider, that one of these protuberancies, being yet manifestly visible, may well be presum'd to consist of a multitude of

of leſſer particles, divers of which may very well be as minute as thoſe Aerial Corpuſcles, that ſucceſſively hit againſt them, and endeavour to carry them along with themſelves. And this may be illuſtrated by a familiar inſtance. For, if you take a lump of *Loaf Sugar*, or even of a much ſolider and harder Body, *Sal Gemmæ*, and caſt it into common water, though this liquor be inſipid, and the motions of its corpuſcles but very languid; yet theſe corpuſcles are capable to looſen and carry off the ſuperficial particles of Sugar or Salt, that chance to lye in their way, and freſh corpuſcles of water ſtill ſucceeding to work upon the remaining particles of the expos'd Body that ſtand in their way, the whole lump is by little and little diſſolved, and ceaſes to appear to the Eye a thing diſtinct from the liquor.

Some things that have occurr'd to me have made me ſuſpect, that 'tis not impoſſible, but that ſome Bodies may receive a diſpoſition to *Volatility*, and

and consequently to pass into the Air by the action either of the Sun-beams, in the form of the Sun-beams, or of some substance that once issued out of the Sun and reach'd unto our Air. For, there may be certain Bodies for the most part in the form of liquors, which, though they pass off from some peculiarly dispos'd Bodies, may during their stay or contact produce in them a great and strange aptness to be volatiliz'd. In favour of which conjecture, I might here alledge *both* the effects, which the *Paracelsians* and *Helmontians* ascribe to the *Alkahest* of volatilizing even fixt and ponderous Bodies barely by being often abstracted from them, *and* some other things, which I shall now leave unmention'd, because you may find them in my Notes about *Volatility* and *Fixity*.

But, whatever become of this *Conjecture*, 'tis consonant to *Experience*, that, either upon the above-recited accounts, or also some others, those parts of the Atmosphere, which, in a stri-

a ſtricter ſenſe, may be call'd the *Air*, are, at leaſt in ſome places, ſo intermixt with particles of differing kinds, that among that great number of various ſorts of them, 'tis very likely that there ſhould be ſome kinds of an un-common and an unobſerved nature. And I could countenance what has been ſaid by the waſting of *Odorous* Bodies, and eſpecially *Camphire*, and by repreſenting, that I have obſerved ſome ſolid Bodies actually cold, when their ſuperficial parts were newly taken off, to emit, though inviſibly, ſuch copious ſteams into the Air, as to grow continually and manifeſtly lighter upon the ballance, ſo as to ſuffer a notable decrement of weight in a minute of an hour. But the mention I make of ſuch things in an other paper, diſſwades me from inſiſting on them here, where 'twill be ſeaſonable to reſume the diſcourſe, which the mention of the *Diſſolving* power, that may be gueſs'd to be in the Air, has for ſome pages interrupted, and to tell you,

you, that thoſe propounded, before I enter'd upon the digreſſion, are the two main Conſiderations *à priori* (as they ſpeak) whereon I have grounded my ſurmize, which being propos'd but as a *Suſpicion*, I preſume it will not be expected, that the Arguments *à poſteriori*, which I ſhall bring to countenance it, ſhould be more than *Conjectures*, much leſs that they ſhould be *Demonſtrations*. And therefore I ſhall venture to lay before you ſome few *Phænomena*, which ſeem to be at leaſt as probably referable to ſome *latent* Quality in the Air, as to any other cauſe I yet know. Upon which ſcore ſuch *Phænomena* may be allowed to be pleaded in favour of our Suſpicion, 'till ſome other *certain* cauſe of them ſhall be ſatisfactorily aſſign'd.

Having premis'd thus much to keep you from looking for ſtronger proofs than I think my task obliges me to give; the *firſt* Phænomenon, I ſhall propoſe, ſhall be the appearing or growth of ſome *Salts* in certain Bodies,

Bodies, which we obſerv'd to afford them either not at all, or at leaſt nothing near in ſuch plenty, or ſo ſoon, unleſs they be expos'd to the Air. Of ſuch a Phænomenon as this, that is not ſo much as mention'd by Vulgar Philoſophers, and very rarely, if at all, to be met with in the Laboratories of Chymiſts, you will not, I ſuppoſe, wonder, that I do not preſent you many Examples, and ſome few I am able to name. For I remember, that ſuſpecting a ſolid Marchaſite, hard as ſtone, to be fit to be made an inſtance for my purpoſe, I caus'd it to be broken, that the internal more ſhining parts might be expos'd to the Air; but, though this were done in a room, where a good fire was uſually kept, ſo that the Marchaſite was not only ſhelter'd from the rain, but kept in a dry Air, yet after a while I diſcover'd upon the gliſtering parts an efflorescence of a *vitriolate* nature.

And afterwards meeting with a ponderous and dark colour'd Mineral,

and

and which, at the firſt breaking, diſcover'd to the Eye no appearance of any Salt, nor ſo much as any ſhining Marchaſitical particles, we found nevertheleſs, that a good quantity of theſe hard and heavy Bodies, being kept expos'd to the Air, even in a room that preſerved them from the rain, though probably they had lain many ages intire in the hill, wherein they were found under ground; yet in not many months, by the operation of the Air upon them, they were, in great part, crumbled to powder exceeding rich in Copperas. Nay, I remember, that having for Curioſities ſake, laid up ſome of theſe ſtones in a room, where I conſtantly kept fire, and in the drawer of a Cabinet, which I did not often take out to give them freſh Air, ſome, if not moſt of them, were notwithſtanding cover'd with a copious efflorescence, which by its conſpicuous colour between blew and green, by its taſte, and by its fitneſs to make in a trice an inky mixture with infuſion of galls,

galls, sufficiently manifested it self to be *Vitriol*; whose growth by the help of the contact of the Air is the more considerable, because it is not a meer Acid Salt, but abounds in Sulphureous and Combustible parts, which I have divers times been able, by Methods elsewhere mentioned, actually to separate or obtain from common Vitriol without the addition of any combustible body, and sometimes without any additament at all. It was also uncommon, that our blackish Minerals requir'd no longer time, nor no rain, to make them afford their *Vitriolate* Efflorescences: For I remember, I kept many of those Marchasites, both glittering ones and others, of which they make and sell great quantities of Vitriol at *Deptford*, without perceiving in them a change that came any thing near to what I have recited. And I observ'd those, whose trade it is to make Vitriol, to be often obliged to let their Vitriol-stones, as they call them, lye half a year, or even eighteen months, or

two years exposed, not only to the open Air, but to the Rain and Sun, to be able to obtain from them their Vitriolate parts.

That also the Earth or Ore of Allum, being robb'd of its Salt, will in tract of time recover it by being expos'd to the Air, we are assur'd by the experienc'd *Agricola*, where, having deliver'd the way of making *Allum*, he subjoins this Advertisement: *Terra Aluminosa, quæ in castellis diluta, postquam effluxit, superfuit egesta et coacervata quotidie, rursus magis & magis fit aluminosa, non aliter atque terra ex qua halinitrum fuit confectum, suo succo plenior fit; quare denuo in Castella conjicitur & aquæ affusæ ea percolantur.*

I have likewise observ'd, as you also perchance have done, that some kind of *Lime* in old walls and moist places has gain'd in length of time a copious efflorescence, very much of a *Nitrous* Nature; as I was convinc'd by having obtain'd *Salt-peter* from it by barely dissolving it in common water,

water, and evaporating the filtrated Solution: And that in calcin'd Vitriol, whose saline parts have been driven away by the violence of the fire, particles of fresh Salt may be found after it has lain a competent time in the Air, I shall e're long have occasion to inform you.

But in the mean time, (to deal ingenuously with you,) I shall freely confess to you, that, though these and the like observations have satisfied Learned men, without having been call'd in question, and consequently have, at least, probability enough to ground our Suspicion upon; yet I, that am more concern'd for the *Discovery* of a *Truth* than the *Reputation* of a *Paradox*, propose the Argument drawn from the foregoing Observations, but as a Probationer. For it yet seems to me somewhat doubtful, *whether* the Salts, that appear in the forementioned cases, are really produc'd by the operation of the Air working as an Agent, or also concurring as an Ingredient; *or* whe-

ther theſe ſaline ſubſtances be not the production of ſome internal thing that is analagous to a Seminal Principle, which makes in theſe bodies a kind of maturation of ſome parts, which being once ripen'd, and perhaps aſſiſted by the moiſture of the Air, diſcloſe themſelves in the form of ſaline Concretions; as in the feculent or Tartareous parts of many Wines there will in tract of time be generated or produc'd ſtore of Corpuſcles of a ſaline nature, that produce the acid taſte we find in *Tartar*, eſpecially that of *Rheniſh* wine. It may alſo be ſuſpected, that the formerly mention'd Salts found in Marchaſites, in Nitrous and Aluminous Earths, *&c.* are made by the ſaline particles of the like nature, that among multitudes of other kinds ſwim in the Air, and are attracted by the congenerous particles, that yet remain in the Terreſtrial bodies, that are, as it were, the wombs of ſuch Minerals, (as I have elſewhere ſhewn, that Spirit of Nitre will, with fixt

Nitre

Nitre and some other Alkalys, compose Salt-peter;) *or* else, that these Aerial Salts, if I may so call them, assisted by the moisture of the Air, do soften and open, and almost corrode or dissolve the more Terrestrial Substance of these wombs, and thereby sollicit out and somewhat extricate the latent Saline particles, and, by their union with them, compose those Emerging bodies that resemble *Vitriol, Allum, &c.*

But not only to suggest these scruples, as if I had a mind they should but trouble you, and keep you irresolute, I shall propound something towards the removal of them; namely, that a convenient quantity of Nitrous Earth, or that other of those Substances, which you would examine, be kept in a close vessel to which the Air has not access, for at least as long time as has been observed to be sufficient to impregnate the like substance, or rather a portion of the same parcel that was chosen to be included: For, if the body, that

was kept close, have either gain'd no Salt at all, or very much less in proportion to its bulk than that which was kept expos'd, we may thence estimate, what is to be ascribed to the Air in the production of Nitre or other saline Concretions. And, because I have observed none of these bodies, that would so soon, and so manifestly, even to the eye, disclose a saline substance, as the blackish Vitriol-Ore, I lately told you I kept in a drawer of my Cabinet; I judg'd *that* a very fit subject, wherewith to try, what maturation or time, when the Air was secluded, would perform towards the deciding of our Difficulty: And accordingly having taken some fragments of it, which we had carefully freed from the adhering Vitriolate efflorescence, by whose plenty we were assured that it was very well dispos'd to be wrought on by the Air, we put of these fragments of differing sizes into two conveniently shap'd glasses, which being Hermetically sealed were ordered

to

to be carried away, and kept in fit places; by which means 'twas expected, that, even without opening the glasses, we should be able easily to see by the chang'd colour of the superficial parts, whether any Vitriolate efflorescence were produced; but, through the negligence or mistake of those, to whom the care was recommended, the experiment was never brought to an issue; and though I afterwards got more of the Mineral, and made a second tryal of the same, I have not yet been inform'd of the event.

But, *Sir*, though, 'till the success of some such tryal be known, I dare not too confidently pronounce about the Production or Regeneration of Salts in bodies that have been robb'd of them, and ascribe it wholly to the Air; yet, when I consider the several and great effects of the Air upon divers other bodies, I think it not rash to conjecture in the mean time, that the operations of the Air may have a considerable share in these

Phænomena, and ſo that there may be *latent* Qualities in the Air, in the ſenſe I declar'd above, where I told you, that, when I ſpeak of theſe Qualities, I look upon them in *Concreto*, (as they phraſe it,) together with the Subſtances or *Corporeal effluvia* they reſide in: And of theſe Aerial Qualities, taken in this ſenſe, I ſhall now proceed to mention ſome other Inſtances.

The Difficulty we find of keeping Flame and Fire alive, though but for a little time, without Air, makes me ſometimes prone to ſuſpect, that there may be diſpers'd through the reſt of the Atmoſphere ſome odd ſubſtance, either of a Solar, or Aſtral, or ſome other exotic, nature, on whoſe account the Air is ſo neceſſary to the ſubſiſtence of Flame; which Neceſſity I have found to be greater, and leſs dependent upon the *manifeſt* Attributes of the Air, than Naturalists ſeem to have obſerved. For I have found by tryals purpoſely made, that a ſmall flame of a Lamp, though

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fed perhaps with a ſubtil thin Oyl, would in a large capacious glaſs-Receiver expire, for want of Air, in a far leſs time than one would believe. And it will not much leſſen the difficulty to alledge, that either the groſs fuliginous Smoak did in a cloſe Veſſel ſtifle the flame, or that the preſſure of the Air is requiſite to impel up the aliment into the wieck: For, to obviate theſe objections, I have in a large Receiver imploy'd a very ſmall wieck with ſuch rectified Spirit of Wine, as would in the free Air burn totally away; and yet, when a very ſmall Lamp, furniſhed (as I was ſaying) with a very ſlender wieck, was made to burn, and, fill'd with this liquor, was put lighted into a large Receiver, that little flame, though it emitted no viſible ſmoak at all, would uſually expire within about one minute of an hour, and, not ſeldom, in a leſs time; and this, though the wieck was not ſo much as ſing'd by the flame: Nor indeed is a wieck neceſſary for the experiment,

ment, ſince highly rectified Spirit of Wine will in the free Air flame away well without it. And indeed it ſeems to deſerve our wonder, what that ſhould be in the Air, which inabling it to keep flame alive, does yet, by being conſum'd or deprav'd, ſo ſuddenly render the Air unfit to make flame ſubſiſt. And it ſeems by the ſudden waſting or ſpoiling of this fine Subject, whatever it be, that the bulk of it is but very ſmall in proportion to the Air it impregnates with its virtue. For after the extinction of the flame, the Air in the Receiver was not viſibly alter'd, and, for ought I could perceive by the ways of judging I had then at hand, the Air retain'd either All, or at leaſt far the greateſt part of its *Elaſticity*, which I take to be its moſt genuine and diſtinguiſhing property.

And this *undeſtroy'd ſpringyneſs* of the Air ſeems to make the neceſſity of freſh Air to the Life of *hot* Animals, (few of which, as far as I can gueſs after many tryals, would be

be able to live two minutes of an hour, if they were totally and all at once deprived of Air,) ſuggeſt a great ſuſpicion of ſome *vital ſubſtance*, if I may ſo call it, diffus'd through the Air, whether it be a *volatile Nitre*, or (rather) ſome *yet anonymous* ſubſtance, Sydereal or Subterraneal, but not improbably of kin to that, which I lately noted to be ſo neceſſary to the maintenance of other flames.

I know not, whether you will think it pertinent to our preſent Diſcourſe, that I obſerve to you, that by keeping putrifying bodies in glaſſes, which by *Hermes* his ſeal were ſecur'd from the contact of the external Air, I have not been able to produce any Inſect or other living Creature, though ſometimes I have kept *Animal* Subſtances and even Blood ſo included for many months, and one or two of them for a longer time; and though alſo theſe Subſtances had a manifeſt change made in their conſiſtence whilſt they remain'd ſeal'd up. On

On this occaſion I ſhall add an odd Obſervation, that I met with in a little Diſſertation *de admirandis Hungariæ aquis*, written by an Anonymous, but Ingenious, Nobleman of that Countrey, where, ſpeaking of the native Salt that abounds in their Regions; he ſays, that in the chief Mine (by them call'd *Deſienſis*) of *Tranſylvania*, there was, a few years before he writ, a great Oak like a huge beam dug out of the middle of the Salt; but, though it was ſo hard, that it would not eaſily be wrought upon by Iron-tools, yet being expos'd to the Air out of the Mine, it became ſo rotten, as he expreſſes it, that in four days it was eaſie to be broken and crumbled between ones fingers. And of that corruptive or diſſolutive Power of the Air near thoſe Mines, the ſame Author mentions other Inſtances.

Having found an Antimonial Preparation to procure Vomits, in a caſe where I did not at all expect it, I was afterwards curious to inquire

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of some Physitians and Chymists, that were of my Acquaintance, whether they had not taken notice, that *Antimonium Diaphoreticum*, which, as its name imports, is wont to work by sweat or transpiration, would not become vomitive, if 'twere not kept from the Air? To which one Physitian, that was a Learned Man, assur'd me, it would, as he had found by particular tryals: And the like answer has been given me by more than one. And I find, that the experienc'd *Zwelfer* himself does somewhere give a caution against letting the Air have access to these Antimonial Medicines, lest it should render them, as he says it will, in tract of time, not only Emetic, but dispos'd to produce heart-burnings, (as they call them,) faintings, and other bad Symptoms. And I learnt by inquiry from a very Ingenious Doctor of Physic, that, having carefully prepar'd *Antimonium Diaphoreticum*, he gave many *doses* of it whilst it was fresh and kept stopt in a glass (without finding

finding that in any Patient it procur'd ſo much as one vomit,) but having kept a parcel of the ſelf ſame Remedy for a pretty while in a glaſs only cover'd looſely with a paper, the Medicine, vitiated by the Air, proved *emetic* (ſtrongly enough) to thoſe, who neither by Conſtitution, or foulneſs of ſtomach, or on any other diſcernable account, were more than others that had taken it diſpoſed to vomit. By which Obſervations, and from what I formerly told you of the Salt-peter obtainable from Quick-lime, a Man partial to the Air would be made forward to tell you, that this looks, as if either there were in the Air a ſubſtance diſpos'd to be aſſimilated by all kinds of bodies, or that the Air is ſo vaſt and rich a Rendevouz of innumerable *ſeminal* Corpuſcles and other Analogous particles, that almoſt any body long expos'd to it may there meet with particles of kin to it, and fit to repair its wrongs and loſſes, and reſtore it to its natural Condition. But without

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out taking any further notice of this odd surmize, I will proceed to mention two or three other *Phænomena* of Nature, that seem to favour the Suspicion, that there may be *Secret* Qualities in the Air in reference to some bodies.

The ingenious *Monsieur de Rochefort*, in the handsom account he gives of the Apple or Fruit of the Tree *Junipa*, whose juice is imploy'd by the *Indians* to black their skins, that they may look the more terrible to their Enemies, observes, that, though the stain, or, as he speaks, the Tincture of this Fruit cannot be wash'd out with Soap, yet within nine or ten days it will vanish of it self; which would make one suspect, that there may be in the Air some secret powerful substance, that makes it a *Menstruum* of more efficacy than Soap it self to obliterate stains. I remember, I have seen this Fruit, but not whilst it was succulent enough to have a tryal made with it; which I was therefore troubled at, because

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the Author does not clearly expreſs, whether this diſappearing of the tincture happens indifferently to the bodies it chances to ſtain, or only is obſerved on the skins of *Men*. For, as in the former Caſe 'twill afford an inſtance pertinent to our preſent purpoſe; ſo in the latter I ſhould ſuſpect, that the vaniſhing of the tincture may be due not ſo much to the operation of the Air upon it, as to the ſweat and exhalations of a human body, which abounding with volatile Salt, may either deſtroy or carry off with them the colour'd particles they meet with in their paſſage.

I have ſometimes, not altogether without wonder, obſerv'd the excellency of the *better* ſort of *Damaſco-ſteel*, (for I ſpeak not of all that goes under that name,) in compariſon of *ordinary* ſteel. And, beſides what I have elſewhere taken notice of concerning it, there is one *Phænomenon*, which though I am not ſure it belongs to the latent Qualities of the Air, yet becauſe it may well do ſo, and I am

am unwilling it ſhould be loſt, I will here tell you, that having inquired of an eminent and experienc'd Artificer, (whom I long ſince imployed in ſome difficult Experiments,) about the properties of *Damaſco-ſteel*, this honeſt and ſober Man averr'd to me, that when he made Inſtruments of it, and gave them the true temper, which is ſomewhat differing from that of other Steel, he generally obſerved, that though, when Raſors or other Inſtruments made of it were newly forged, they would be ſometimes no whit better, and ſometimes leſs good, than thoſe made of other Steel; yet when they had been kept a year or two or three in the Air, though nothing elſe were done to improve them, they would be found much to ſurpaſs other Inſtruments of the ſame kind, and what themſelves were before; in ſo much, that ſome of them have been laid aſide at firſt, as no way anſwering the great expectation conceived of them, which after two or three years were found

to ſurpaſs it; of which alſo I am now making a tryal. I have ſeveral times made a ſubſtance that conſiſts chiefly of a Metalline body, and is of a texture cloſe enough to lye for many hours undiſſolv'd in a Corroſive Menſtruum; and yet this ſubſtance, that was fixt enough to endure the being melted by the Fire without loſing its colour, would, when I had purpoſely expos'd it to the Air, be diſcoloured in a very ſhort time, and have its ſuperficial parts turned almoſt black.

And this brings into my mind that very pretty Obſervation, that has been newly made in *Italy* by an ingenious Man, who took notice, that, if after the opening of a Vein the blood be kept 'till it be concreted, and have excluded the ſuperficial *ſerum*, though the lower part be uſually of a dark and blackiſh colour in compariſon of the ſuperficial parts, and therefore be counted far more feculent; yet, if the lump or clott of blood be broken, and the internal

and dark coloured parts of the blood be expos'd to the Air, it will after a time (for 'tis not said how long) be so wrought on by the contact of the Air, that the superficial part of the blood will appear as florid as the lately mention'd upper part (suppos'd to be, as it were, the flower of the blood,) did seem before. And this Observation I found to hold in the blood of some *Beasts*, whereon I tryed it, in which I found it to succeed in much fewer minutes, than the *Italian Virtuoso*'s Experiment on *Human* blood would make me expect.

On the other side I have often prepar'd a Substance, whose effect appears quite contrary to this. For, though this factitious Concrete, whilst kept to the Fire or very carefully preserved from the Air, be of a red colour almost like the common opacous *Bloodstone* of the shops; yet, if I broke it, and left the lumps or fragments of it a little while in the Air, it would in a short time (sometimes perhaps not amounting to a

quarter of an hour) it would, I ſay, have its ſuperficial parts turn'd of a very dark colour, very little, and ſometimes ſcarce at all, ſhort of blackneſs.

A very inquiſitive Perſon of my acquaintance, having occaſion to make, by Diſtillation, a Medicine of his own deviſing, chanc'd to obſerve this odd property in it, That at that time of the year, if it were kept ſtopt, it would be coagulated almoſt like Oyl of Anniſeeds in cold weather; yet, if the ſtopple were taken out, and ſo acceſs were for a while given to the Air, it would turn to a liquor, and the veſſel being again ſtopt, it would, though more ſlowly, recoagulate. The hints, that I gueſs'd might be given by ſuch a Phænomenon, making me deſirous to know ſomething of it more than barely by Relation, I expreſs'd rather a curioſity than a diffidence about it; and the Maker of it telling me, he thought, he had in a ſmall Vial about a ſpoonful of this Medicine left in a neigh-

neighbouring Chamber, I desired his leave to consider it my self, which Request being presently complied with, I found it, when he brought it into the Room which I stayed in, not liquid but consistent, though of but a slight and soft contexture. And having taken out the Cork, and set the Vial in a window, which (if I well remember) was open, though the Season, which was Winter, was cold, yet in a little time that I stayed talking with the Chymist, I found, that the so lately coagulated substance was almost all become fluid. And another time, when the Season was less cold, having occasion to be where the Vial was kept well stopt, and casting my Eyes on it, I perceiv'd the included substance to be coagulated much like Oyl of Anniseeds. And this substance having, as the Maker assur'd me, nothing at all of Mineral in it, nor any Chymical Salt, it consisting only of two simple bodies, the one of a vegetable and the other of an animal substance, distill'd toge-

ther, I ſcarce doubt but you will think with me, that theſe contrary operations of the Air, which ſeems to have a power in ſome Circumſtances to coagulate ſuch a body, and yet to diſſolve and make it fluid when freſh and freſh parts are allow'd acceſs to it, may deſerve to be further reflected on, in reference (among other things) to the opportune operations, the inſpired Air may have on the conſiſtence and motion of the circulating blood, and to the diſcharge of the fuliginous recrements to be ſeparated from the blood in its paſſage through the Lungs.

There are two other *Phænomena* that ſeem favourable to our Suſpicion, *That there are Anonymous Subſtances and Qualities in the Air*, which ought not to be altogether prætermitted on this occaſion; though, becauſe to ſpeak fully of them would require far more time than I can now ſpare, I ſhall ſpeak of them but ſuccinctly.

The *latter* of theſe two *Phænomena* is

is the growth or appearing production of *Metals* or Minerals dug out of the Earth, and expos'd to the Air. And this, though it be the last of the two, I mention first, because it seems expedient, lest it should prove too long an interruption to our Discourse, to postpone the Observations and annex them to the end of this Paper; only intimating to you now, that the caution I formerly interposed about the Regeneration of *Salts* in Nitrous and other Earths, may, for greater security, be applied, *mutatis mutandis*, to that production of Metalline and Mineral bodies we are speaking of.

The *other* of the two *Phænomena*, I lately promis'd to mention, is afforded me by those various and odd *Diseases*, that at some times and in some places happen to invade and destroy numbers of *Beasts*, sometimes of one particular kind, and sometimes of another. Of this we have many instances in the Books of approved Authors, both Physitians and others;

and I have my ſelf obſerv'd ſome notable Examples of it. But yet I ſhould not mention it as a ground of Suſpicion, that there may be, in ſome times and places, unknown *Effluvia* and powers in the Air, but that I diſtinguiſh theſe from thoſe Diſeaſes of Animals, that proceed, as the Rot in Sheep often does, from the exorbitancy of the Seaſons, the immoderateneſs of Cold, Heat, or any other manifeſt Quality in the Air. And you will eaſily perceive, that ſome of theſe Examples probably argue, that the Subterraneal parts do ſometimes (eſpecially after Earthquakes or unuſual cleavings of the ground) ſend up into the Air peculiar kinds of venomous Exhalations, that produce new and mortal Diſeaſes in Animals of ſuch a *ſpecies*, and not in thoſe of another, and in this or that particular place, and not elſewhere: Of which we have an eminent Inſtance in that odd Plague or Murrain of the year 1514, which *Fernelius* tells us invaded none but Cats. And even

even in Animals of the same *species*, sometimes one sort have been incomparably more obnoxious to the Plague than another; as *Dionysius Halicarnasèus* mentions a Plague that attack'd none but *Maids*; whereas the Pestilence that raged in the time of *Gentilis* (a fam'd Physitian) kill'd few *Women*, and scarce any but lusty Men. And so *Boterus* mentions a great Plague, that assaulted almost only the younger sort of persons, few past thirty years of age being attack'd by it: Which last Observation has been also made by several later Physitians. To which may be added, what Learned Men of that Faculty have noted at several times concerning Plagues, that particularly invaded those of this or that Nation, though confusedly mingled with other People; as *Cardan* speaks of a Plague at *Basil*, with which only the *Switzers*, and not the *Italians*, *French*, or *Germans*, were infected. And *Johannes Utenhovius* takes notice of a cruel Plague at *Copenhagen*, which, though it raged among

among the *Danes*, ſpared both the *Engliſh*, *Dutch*, and *Germans*, though they freely enter'd infected houſes, and were not careful to ſhun the ſick. In reciting of which Inſtances I would not be underſtood, as if I imputed theſe effects meerly to noxious Subterraneal fumes; for I am far from denying, that the peculiar Conſtitutions of Mens Bodies are likely to have a great intereſt in them: But yet it ſeems leſs probable, that the peſtilent venom diffuſed through the Air ſhould owe its enormous and fatal efficacy to the exceſs of the *manifeſt* Qualities of the Air, than to the peculiar nature of the peſtilential poiſon ſent up into the Air from under ground, which when it is by dilution or diſſipation enervated, or by its progreſs paſt beyond the Air we breath in, or render'd ineffectual by ſubterraneal or other Corpuſcles of a contrary Quality, the Plague, which it, as a con-cauſe, produced, either quite ceaſes, or degenerates into ſomewhat elſe. But I have not

time to countenance this Conjecture, much less to consider, whether some of those Diseases, that are wont to be call'd *new*, which either did begin to appear, or at least to be rife, within these two or three Centuries, as the *Sudor Anglicus* in the fifteenth Century, the *Scurvy*, and the *Morbus Hungaricus*, the *Lues Moraviæ*, *Novus Morbus Luneburgensis*, and some others, in the last Century of all, may be in part caus'd by the exotic steams this Discourse treats of. But this Consideration I willingly resign to Physitians.

And now, if the two forementioned Suspicions, the one about *Subterraneal*, the other about *Sydereal*, Effluviums, shall prove to be well grounded, they may lead us to other Suspicions and further thoughts about things of no mean Consequence; three of which I shall venture to make mention of in this place.

I. For we may hence be awakened to consider, whether divers changes of Temperature and Constitution in the

the Air, not only as to *manifeſt* Qualities, but as to the more *latent* ones, may not ſometimes in part, if not chiefly, be derived from the paucity or plenty, and peculiar nature of one or both of theſe ſorts of Effluviums. And in particular, we find in the moſt approved Writers ſuch ſtrange *Phænomena* to have ſeveral times happen'd in great Plagues and contagious Diſeaſes, fomented and communicated, nay (as many eminent Phyſitians believed) begun, by ſome *latent* peſtiferous, or other malignant, *Diatheſis* or Conſtitution of the Air, as have obliged many of the Learned'ſt of them to have recourſe to the immediate operation of the Angels, or of the Power and Wrath of God himſelf, or at leaſt to ſome unaccountable influence of the Stars; none of the Solutions of which difficulties ſeems preferable to what may be gathered from our Conjecture; ſince of Phyſical Agents of which we know nothing ſo much, as that they are to us inviſible and

probably

probably of a heteroclite nature, it need be no great wonder, that the operation ſhould alſo be abſtruſe, and the effects uncommon. And on this occaſion it may be conſider'd, that there are clearer inducements to perſwade us, that another Quality of the Atmoſphere, its *Gravity*, may be alter'd by unſeen Effluviums, aſcending from the Subterraneous Regions of our Globe; and we have often perceived by the Mercurial *Baroſcope* the *Weight* of the Air to be notably increaſed, when we could not perceive in the Air nor ſurface of Earth any cauſe to which we could aſcribe ſo notable a change. And this gives me a riſe to add, that I have ſometimes allowed my ſelf to doubt, whether even the *Sun* it ſelf may not now and then alter the *Gravity* of the Atmoſphere otherwiſe than by its Beams or Heat. And I remember, I deſired ſome *Virtuoſi* of my acquaintance to aſſiſt me in the inquiry, whether any of the *Spots*, that appear about the Sun, may not,

upon

upon their sudden dissolution, have some of their discuss'd and dispers'd matter thrown off, as far as to our Atmosphere, and that copiously enough to produce some sensible alterations in it, at least as to Gravity.

II. Another thing, that our two forementioned Suspicions, if allow'd of, will suggest, is, that it may not seem altogether improbable, that some bodies, we are conversant with, may have a peculiar disposition and fitness to be wrought on by, or to be associated with, some of those exotic Effluvia, that are emitted by unknown bodies lodged under ground, or that proceed from this or that Planet. For what we call Sympathies and Antipathies, depending indeed on the peculiar Textures and other Modifications of the bodies, between whom these friendships and hostilities are said to be exercised, I see not why it should be impossible, that there be a Cognation betwixt a body of a congruous or convenient Texture, (especially as to the shape

and

and size of its Pores,) and the Effluviums of any other body, whether Subterraneal or Sydereal. We see, that convex Burning-glasses, by virtue of their figure and the disposition of their pores, are fitted to be pervaded by the beams of Light and to refract them, and thereby to kindle combustible matter; and the same beams of the Sun will impart a lucidness to the *Bolonian* stone. And as for *Subterraneal* bodies, I elsewhere mention two Minerals, which being prepared, (as I there intimate,) the steams of the one, ascending without adventitious Heat and wandering through the Air, will not sensibly work on other bodies; but if they meet with that which we prepared, they will immediately have an operation on it, whose effect will be both manifest and lasting.

See the Experiment in the Discourse of the Determinate Nature of Effluviums.

III. I now pass on to the other thing, that the two formerly mentioned Suspicions may suggest, which is,

is, that if they be granted to be well founded, we may be allow'd to consider, whether among the bodies we are acquainted with here below, there may not be found some, that may be *Receptacles*, if not also *Attractives*, of the Sydereal, and other exotic Effluviums that rove up and down in our Air.

Some of the Mysterious Writers about the *Philosophers-stone*, speak great things of the excellency of what they call their *Philosophical Magnet*, which, they seem to say, attracts and (in their phrase) corporifies the *Universal Spirit*, or (as some speak) the *Spirit of the World*. But these things being abstrusities, which the Writers of them profes's'd to be written *for*, and to be understood only *by*, the *Sons of Art*; I, who freely acknowledge I cannot clearly apprehend them, shall leave them in their own worth as I found them, and only, for brevity sake, make use of the receiv'd word of a *Magnet*, which I may do in my own sense, without avow-

avowing the receiv'd Doctrine of Attraction. For by such a Magnet, as I here purpose to speak of, I mean not a body that can properly attract our foreign Effluviums; but such an one, as is fitted to detain and join with them, when by virtue of the various motions, that belong to the Air as a Fluid, they happen'd to accost the Magnet. Which may be illustrated by the known way of making *Oyl of Tartar* (as the Chymists call it) *per Deliquium*. For, though the Spagyrists and others suppose, that the fiery Salt draws to it the Aqueous Vapours, yet indeed it does but arrest, and imbody with, such of those that wander through the Air, as chance in their passage to accost it.

And, without receding from the *Corpuscularian* Principles, we may allow some of the bodies, we speak of, a greater resemblance to Magnets, than what I have been mentioning. For not only such a Magnet may upon the bare account of Adhesion by

Juxta-poſition or Contact, detain the Effluviums that would glide along it, but theſe may be the more firmly arreſted by a kind of precipitating faculty, that the Magnet may have in reference to ſuch Effluviums; which, if I had time, I could illuſtrate by ſome Inſtances; nay I dare not deny it to be poſſible, but that in ſome Circumſtances of time or place one of our Magnets may, as it were, *fetch in* ſuch ſteams as would indeed paſs near it, but would not otherwiſe come to touch it. On which occaſion I remember, I have in certain caſes been able to make ſome bodies, not all of them *Electrical*, attract (as they ſpeak) without being excited by rubbing, *&c.* far leſs light bodies, than the Effluviums we are ſpeaking of.

But this it may ſuffice to have glanc'd at, it not being here my purpoſe to meddle with the myſtical Theories of the Chymiſts; but rather to intimate, that, without adopting or rejecting them, one may diſcourſe

discourse like a Naturalist about Magnets of Celestial and other Emanations, that appear not to have been consider'd, not to say, thought of, either by the Scholastic, or even the Mechanical, Philosophers.

OF CELESTIAL & AERIAL *MAGNETS.*

IF now, upon what I have granted in the close of the past Discourse, you should urge the question further, and press me to declare, Whether, as I think it no impossible thing, that *Nature* should *make*, so I think it no unpracticable or hopeless thing, that *Men* should *find*, or *Art* should *prepare*, useful *Magnets* of the exotic Effluviums of the lower region of the Earth, or the upper of the World: It would much distress me to give any other answer, than that I think it extreamly difficult, and not absolutely impossible; and therefore I would not discourage any curious or

induſtrious Man from attempting to ſatisfie himſelf by Experiments, becauſe even a ſeemingly ſlight diſcovery in a thing of this nature may be of no ſmall uſe in the inveſtigation *of* the nature of the Air, eſpecially in ſome particular places, and *of* the Correſpondency, which, by the intervention of the Air, the ſuperficial part of the Terreſtrial Globe may have both with the *Subterraneal* Regions of the Earth, and the *Celeſtial* ones of the Univerſe. Some of the things I have tryed or ſeen relating to this diſcovery, I muſt for certain reaſons leave here unmentioned, and only advertiſe you, that ſeveral bodies, which experience has aſſur'd us do imbibe or retain ſomething from the Air, as ſome calcin'd Minerals, ſome Marchaſites, ſome Salts, as well factitious as natural, *&c.* may be fit to be often expoſed to it, and then weighed again, and farther diligently examined, whether that which makes the increment of weight, be a meer imbibed moiſture

or

or also somewhat else; and likewise whether it be separable from the body or not, or however have endowed it with any considerably Quality; and if you chance to meet with a good Magnet, you may then vary Experiments with it, by exposing it long to the Air in Regions differing much in Climat, or Soil, or both, by exposing it by day only, or by night, at several Seasons of the Year, in several Temperatures of the Air, at several considerable Aspects of the Stars and Planets, by making it more or less frequently part with what it has gain'd from the Air; and in short, by having regard to variety of Circumstances, which your Curiosity and Sagacity may suggest. For, by thus diversifying the Experiment many ways, you may perhaps, by one or other of them, make some unexpected and yet important discovery of what Effluviums the Air, in particular places and times, abounds with, or wants, and perchance too, of some correspondency between the

Terreſtrial and Etherial Globes of the World.

I ſhall neither be ſurpriz'd nor quarrel with you, if you tell me, that theſe are extravagant thoughts; but if I had been fortunate in preſerving all, that Tryal, Obſervation, or other productions of ſome Curioſity, I once had for ſuch Inquiries, procur'd me, you would not perhaps think me ſo *very* extravagant. But though I muſt not here make any further mention of them, and ſhall only take notice of one body, namely *VITRIOL*, *whether* crude, *or* unripe, and (as Chymiſts ſpeak) embrionated, *or* Spagyrically prepar'd; yet ſome *Phænomena* of theſe Vitriolate Subſtances may for the preſent, I hope, ſomewhat moderate your cenſure for my putting you upon Obſervations that I fear you your ſelf will judge unpromiſing, and leſs favourable perſons than you would think phantaſtical. And to let you ſee by a pregnant Inſtance, that the Air may not only have a Notable operation

tion upon *Vitriol*, and that, after a ſtrong fire could work no farther on it, but that this operation was conſiderably diverſified by Circumſtances; I ſhall begin what I have to alledge, with what the experienc'd *Zwelfer* occaſionally obſerv'd, and relates to uſher in a caution about a Chymical Preparation of *Vitriol*: For, having inform'd his Reader, that the *Colcothar*, that is made by a ſtrong Diſtillation, is not corroſive, he denies, that, (to uſe his own words) *ſtatim à Diſtillatione Sal ex eodem, affuſâ aquâ, elici queat; ſed tum prius*, (continues he) *ubi aliquandiu aeri expoſitum fuerit; tunc enim ſal præbet quandoque candidum, quandoque purpureum, aſpectu pulcherrimum, quod aliquando in copia acquiſivi, & penes me aſſervo, quandoque etiam Nitroſum.*

Which Teſtimony of this candid Spagyriſt has much the more weight with me, becauſe I find, what he affirms of the Saltleſneſs of newly and ſtrongly calcin'd Vitriol to be very agreeable to ſome of my Experiments

ments about *Colcothar* of blew (venereal) Vitriol; which Salt or Mineral (I mean Vitriol) is so odd a Concrete, that I have thought fit more than once to recommend the making Experiments about it to several Curious Persons, that had better opportunity to continue them than I, whose residence was not so fixt. And I remember, that one of these, a Person industrious and versed in Chymical Operations, gave me this account, that not only he had differing kinds of Salts from Colcothar expos'd to the Air for many months, and robb'd at convenient times of what it had acquir'd, but that in tract of time he found it so alter'd, that he obtain'd from it a pretty quantity of true running Mercury.

And now, to resume and conclude what I was saying about Colcothar, there are two or three things I would propose to be observed by you, or any *Virtuoso* that would assist me in these tryals about this odd *Calcinatum*, (for to call it *Terra damnata*, were to injure it.) The

The *first* is, to take notice of some Circumstances that most Observers would overlook; such as (besides the Nature of the Soil) the Temperature of the Air, the Month of the Year, and the Winds, the weight of the Atmosphere, the Spots of the Sun, if any be, the Moons Age, and her Place in the *Zodiack*, and the principal Aspects of the Planets, and the other chief Stars. For, though it be a boldness to *affirm*, that any, or perhaps all of these together, will have any interest in the production of the Salt or other Substance, to be made or disclosed in the Colcothar; yet in things new and exorbitant, it may be sometimes rash and peremptory to *deny*, even such things as cannot, without rashness, be positively asserted; and in our case the small trouble of taking notice of Circumstances will be richly paid by the least discovery made in things so abstruse and considerable. And as we cannot yet knowingly pronounce, so much as *negatively*, whether the *Libration* of the

the *Moon* and the *Motion* of the *Sun* (and perhaps of ſome of the other Planets) about their own Centers, and conſequently their obverting ſeveral parts of their bodies to us, may have an operation upon our Atmoſphere; ſo, for ought I know, there may be in thoſe vaſt internal parts of the Earth, whoſe thin cruſt only has been here and there dug into by Men, conſiderable Maſſes of Matter, that may have periodical Revolutions, or Accenſions, or Eſtuations, or Fermentations, or, in ſhort, ſome other notable Commotions, whoſe Effluvia and Effects may have operations, yet unobſerved, on the Atmoſphere and on ſome particular bodies expos'd to it; though theſe periods may be perhaps either altogether irregular, or have ſome kind of regularity differing from what one would expect. As we ſee, that the Sea has thoſe grand Intumeſcencies, we call *Spring-tides*, not every day, nor at any conſtant day of the month or week, but about the Full and New Moon; and theſe Spring-

Spring-tides are most notably heighten'd, not every month, but twice a year, at or about the Vernal and Autumnal Equinoxes; which Observations have not been near so antient and known, as the daily Ebbing and Flowing of the Sea. The *Etesians* of the Antients I shall not now insist on; nor the Observations that I think I elsewhere mention'd of the Elder Inhabitants of the *Caribe*-Islands, who, when the *Europeans* first resorted thither, were wont to have *Hurricanes* but once in seven years; afterwards they were molested with them but once in three years; and of late they are troubled with them almost every year. And a Physitian that lived there told me, that he had scarce ever observed them to come but within the compass of two months joyning to one another. In which Instances, and divers others that may be noted of what changes happen'd to great Quantities of Matter, Nature seems to affect something of periodical, but not in a way

way that appears to us, regular.

One may add on this occasion that memorable passage related by the Learned *Varenius* of those Hot Springs in *Germany*, that he calls *Thermæ Piperinæ*, of which he affirms in more than one place, that they have this peculiarity, that they annually begin and cease to flow at certain times; the former about the third day of *May*, and the latter near the middle of *September*, at which time they are wont to rest till the following Spring. But though, for ought I know, our Geographers Observation will hold in hot Springs; yet it must not be extended to all, at least, if we admit that which is related by the accurate *Johannes de Laet*, (I suppose out of *Ximenes*, or the famous Conquerour of *Mexico*, *Cortes*,) who tells us, that in the *Mexican* Province, *Xilotepec*, *Fons celebratur, qui quatuor continuis annis*

Varenius. *Lib.* 1. *Geograph. Univers. Thermæ omnes ferè quas novimus sine cessatione fluunt exceptis Piperinis Germaniæ, &c.*

Amer. Lib. V. cap. 7.

annis scaturit, deinde quatuor sequentibus deficit, & rursus ad priorem modum erumpit, &, quod mirabile, pluviis diebus, parciùs, quum sudum est tempus & aridum, copiosiùs, exuberat.

But this is not a place to enlarge upon the grounds of my suspecting, there may be some periodical Motions and Commotions within the Terrestrial Globe; what has been mention'd being only to invite you to take notice of Circumstances in your Observations of *Colcothar*, some of which may, with the more shew of probability, be kept expos'd for a long time, because that Bars of Windows and other erected Irons I have found to acquire in tract of time from the *Effluvia* of the Earth a settled Magnetism.

The *other* main thing I would recommend, is, that notice be taken *not only* of the kind of Vitriol, the Colcothar is made of; (for I generally used blew *Danzig Vitriol*) as *Martial Vitriol*, *Hungarian Vitriol*, *Roman Vitriol*, *&c.* to which I have, for Curi-

Curiosity, added Vitriol made by our selves of the Solution of the more saline parts of Marchasites in water, without the usual additament of Iron, or Copper; *but also*, to what degree the calcination is made; and how far the calcin'd Matter is freed from the Salt by water. For these Circumstances, at least in some places, may be of moment, and perhaps may afford us good hints of the Constitution of the Atmosphere in particular parts, as well as of the best preparation of Colcothar for detaining the exotic Effluviums. And I would the rather have Experiments tryed again in other places with Colcothar not calcin'd to the utmost, nor yet so exquisitly edulcorated, but that some saline particles should be left in it for future increase; because I have more than once purposely tryed in vain, that the *Caput Mortuum* of blew Vitriol, whereof the Oyl and other parts had been driven off with a violent and lasting fire, would not, when fresh, impart any saltness

to

to the water; nor do I think, that out of some ounces purposely edulcorated I obtained one grain of Salt. And this saltless Colcothar being expos'd, some by me, and some by a Friend that had conveniency in another place not far off, to the Air, some for many weeks and some for divers months, we did not find it to have manifestly increased in weight, or to have acquired any sensible saltness, which, supposing the Vitriol to have nothing extraordinary, gave me the stronger suspicion of some peculiarity in the Air of that part of *London*, where the Tryals had been made, at least during those times wherein we made them; because not only former experience, made here in *England*, had assur'd me, that some Colcothars will gain no despicable accession of weight by being expos'd to the Air; but accidentally complaining of my lately mention'd disappointment to an ingenious Traveller, that had, in divers Countries, been curious to examine their Vitriols, he

assured me, that, though he usually dulcified his Colcothar very well, yet within four or five weeks he found it considerably impregnated by the Air 'twas exposed to.

It remains, that I add one intimation more about Vitriol, which is, that I have found it to have so great a correspondency with the Air, that it would not be amiss to try, not only Colcothar of differing Vitriols (whether barely made the common way, or without any Metalline addition to the Vitriol Stones or Ore,) but other Preparations of Vitriol too, such as exposing Vitriol, only calcin'd to whiteness by the Sun-beams, or further to an higher colour by a gentle Heat, or throughly calcined, and then impregnated with a little of its own Oyl. For such Vitriolate Substances as these, the Air may work upon, nay even liquid Preparations of Vitriol may be peculiarly affected by the Air, and thereby perhaps be useful to discover the present constitution, or foretel some approaching

ching changes of it. Of the use of which conjecture, namely the peculiar action of the Air on some Vitriolate Liquors, I remember I shew'd some *Virtuosi* a new Instance in an Experiment, whereof this was the Sum: [I elsewhere mention a Composition that I devis'd, to make with Sublimate, Copper, and Spirit of Salt, a Liquor of a Green exceeding lovely. But in the description of it I mention'd not (having no need to do it there) a circumstance as odd as the liquor it self was grateful. For the Air has so much interest in the production of this green, that when you have made the Solution of the Copper and Mercury with the Spirit of Salt, that Solution will not be *green*, nor so much as *greenish*, as long as you keep it stopt in the bolt-head, or such like glass wherein 'tis made. But if you pour it out into a Vial, which, by not being stopt, leaves it expos'd to the Air, it will after a while sooner or later attain that delightful green that so much endears it to the Behol-

ders Eye. This appear'd ſo odd an Experiment to the *Virtuoſi*, to whom I firſt related it, that thoſe that could not gueſs by what means I attain'd it, could ſcarce believe it. But that troubled not me, who, to ſatisfie my ſelf not only of the Truth of the Experiment, but that 'twas not ſo contingent as many others, repeated it ſeveral times, and found the Solution, 'till the *Air* made it floriſh, to be of a muddy reddiſh colour quite differing from green. So that I remember, that having once kept ſome of the liquor in the ſame glaſs-egg, wherein the Solution had been made, it look'd like very dirty water, whilſt the other part of the ſame Solution, having been expos'd to the Air, emulated the colour of an Emerald. In which change 'tis remarkable, that to clarifie this liquor and give it a tranſparent greeneſs, I perceiv'd not, that any precipitation of foul matter was made to which the alteration could be aſcrib'd; and yet to make it the more probable that *this*

change

change proceeded not from a subsidence made of some opacating matter effected by some days rest, I kept some of the Solution seal'd up in a fine Vial several months, without finding it at the end of that time other than a dark or muddy liquor, which, in short time, it ceas'd to be, when, the Hermetic Seal being broken off, the Air was permitted to work upon it. And this I further observ'd in our various Experiments on this liquor, that, according to the quality of the matter and other Circumstances, the greeneſs was not attain'd to but at certain periods of time, now and then discloſing it self within two or three days, and sometimes not before nine or ten.]

With how little Confidence of succeſs Tryals, that have the aimes of theſe I have been speaking of, are to be attempted, not only consideration but experience have made me senſible. But yet I would not discourage Mens Curioſity from venturing even upon slight probabilities,

where the Nobleness of the Subjects and Scope may make even small attainments very desirable. And 'till tryal have been made on occasions of great moment, 'tis not easie to be satisfied, that Men have not been wanting to themselves; which I shall only illustrate by proposing, what, I presume, will not need that I should make an application of it. Those adventurous Navigators, that have made Voyages for Discovery in unknown Seas, when they first discern'd something obscure near the Horizon at a great distance off, have often doubted, whether what they had so imperfect a sight of, were a Cloud, or an Island, or a Mountain: But though sometimes it were more likely to be the former, as that which more frequently occurr'd, than the latter; yet they judg'd it advisable to steer towards it, 'till they had a clearer prospect of it: For if it were a deluding Meteor, they would not however sustain so great a loss in that of a little labour, as, in case it were a Coun-

a Country, they would in the loss of what might prove a rich Discovery: And if they desisted too soon from their Curiosity, they could not rationally satisfie themselves, whether they slighted a Cloud or neglected a Country.

FINIS.

OBSERVATIONS

ABOUT THE

GROVVTH

OF

METALS in their ORE

Expoſed to the AIR.

By the Honourable

ROBERT BOYLE,

Fellow of the *Royal Society*.

LONDON,

Printed by *William Godbid*, and are to be Sold by *Moſes Pitt*, at the *Angel* over againſt the little North Door of St. *Paul's* Church. 1674.

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OBSERVATIONS OF THE GROWTH OF METALS.

IT is altogether unnecessary to my present purpose, to examin, whether Metals and Minerals, as if they were a kind of subterraneal Plants, do properly grow as Vegetables do. For this Inquiry belongs to another place, but not to this, where the reference made in the 39th page of the foregoing Paper does not oblige me to speak of the *Growth of Metals* in any other than a lax and popular sense, in which a Metal may be said to grow, if a portion of

Matter being assign'd, wherein as yet Men can find either no Metal, as Gold or Tin, or but such a quantity of it; this being expos'd to the Air, will after a time either afford some Metal where there appear'd none before, or a greater proportion of Metal than it had before.

Observations of this kind requiring length of time, as well as residence near places abounding with Minerals, I have little or no opportunity to make any of them my self, at least with the wariness, that to me seems due to Observations that I think not easie to be well made. And therefore I must content my self to set down what I have been able to learn by conversing with Mineralists and Travellers, and to add some particulars that I met with in Authors of good Credit.

OB-

OBSERVATIONS ABOUT THE Growth of TIN.

AN ancient Owner of Mines, being asked by me, Whether he could, otherwise than upon the Conjectures of vulgar Tradition, prove, that Minerals grow even after the Veins have been dug? Answer'd affirmatively; and being desired to let me know his proofs, he gave me these that follow.

He told me, that not far from his House there was a *Tin-Mine*, which the old Diggers affirm'd to have been left off, some said eighty, some an hundred & twenty years ago, because they had by their washing and vanning separated all the Ore from the rest of the Earth, and yet of late years they

found it ſo richly impregnated with Metalline Particles, that it was wrought over again with very good profit, and preferr'd to ſome other Mines that were actually wrought, and had never been ſo robb'd. And when I objected, that probably this might proceed from the lazineſs and unskilfulneſs of Workmen in thoſe times, who left in the Earth the Tin that was lately ſeparated, and might then have been ſo; I was anſwer'd, that 'twas a known thing in the Country, that in thoſe times the Mine-men were more careful and laborious to ſeparate the Metalline part from the reſt of the Ore, than now they are.

He alſo affirmed to me, that in his own time ſome Tenants and Neighbours of his (imploy'd by him) having got all the Ore they could out of a great quantity of ſtuff, dug out of a Tin-Mine, they laid the remains in great heaps expos'd to the Air, and within twenty and thirty years after, found them ſo richly im-

impregnated, that they wrought them over again with good benefit.

And lastly he assured me, that, in a Work of his own, wherein he had exercis'd his skill and experience, (which is said to be very great) to separate all the particles of the Tin from the Terrestrial substances, that were dug up with it out of the Vein, he caus'd Dams to be made to stop the Earthy Substance, which the Stream washed away from the Ore, giving passage to the water after it had let fall this Substance, which lying in heaps expos'd to the Air, within ten or twelve years, and sometimes much less, he examin'd this or that heap, and found it to contain such store of Metalline particles, as invited him to work it again and do it with profit. And yet this Gentleman was so dexterous at separating the Metalline from the other parts of Tin-Ore, that I could (not without wonder) see what small Corpuscles he would, to satisfie my Curiosity, sever from vast quantities (in propor-

tion) of Earthy and other Mineral stuff.

Relations agreeable to these, I received from another very ingenious Gentleman that was conversant with Tin-Mines; and lived not far from more than one of them.

I was the more solicitous to procure an information about the Growth of this Metal, because the bulk of that, which is us'd in *Europe*, being found in *England*, I have met with little or no mention of the Growth of it in Outlandish Writers.

OBSERVATIONS
ABOUT THE
Growth of LEAD.

AS for the Growth of *Lead* in the Ore expos'd to the Air, I remember, I enquir'd about it of a Person of Quality, who had a Patent for divers Leaden Mines that were suppos'd to contain Silver, and wrought some of them himself at no small charge, yet not without profit; and, as I remember, he answer'd me, that the *Lead-Ore*, that had been wrought and laid in heaps, did, in tract of time, grow impregnated with Metal again, and, as experience manifested, became worth working a second time. And indeed some Mineralists deliver it as a general Observation, that the Growth and Renascence of Metals is more

more manifest in *Lead* than in any other of them. *Fessularum mons in Hetruria*, says *Boccatius Certardus*, who delivers it as a most approved Truth, *Florentiæ Civitati imminens, lapides plumbarios habet, qui si excidantur brevi temporis spatio novis Incrementis instaurantur.* J. Gerhard. *in Decade quæstionum, pag. m.* 22.

Tu subtilius ne quæras (says *Agricola*, speaking of the Growth of Mines in general) *sed tantummodo refer animum ad cuniculos, & considera, eos adeò interdum memoriâ hominum in angustum venisse, ut aliqua sui parte nullum aut admodum difficilem præbeant transitum, cùm eos satis latè agere soleant Fossores, ne transituros impediant. In tales autem angustias sunt adducti propter accretionem materiæ ex qua lapis est factus.*

But whether this increment of Lead is observable in all Mines of that Metal, I was induc'd to doubt by the answer given me by a Gentleman, whose House was seated near several *Lead-Mines*, and who was himself Owner of one or two, which he yet

causes

causes to be wrought: For this Gentleman, though a Chymist, assured me, that in the Country where he lives, which is divided by the Sea from that of the Person above-mention'd, he never observ'd the Lead-Ore to increase, either out of the Veins or in them; but that in some places, whence Ore had been dug thirty or forty, if not fifty, years before, he perceived not on the sides of the passages, whence the Ore had been dug, that any other had grown in its place, or that the passages, though narrow before, were sensibly straighten'd, much less block'd up.

And indeed, if there were no other Arguments in the case, the straightning of the ancient passages in process of time would not convince me. For, when I consider, that the Soils that abound with Metals do usually also abound with waters, which are commonly imbibed by the neighbouring Earth; and when I consider too, that water is somewhat expanded by being turned into

into Ice, and that this expansion is made, (as I have often tryed) though slowly, yet with an exceeding great force, by which it often stretches or breaks the Vessels that contain it: When I consider these things, *I say*, I am apt to suspect, that sometimes the increasing narrowness of the subterraneal passages in Mines may proceed from this, that the Soil that invirons them, if they lye not deep, may have the water, imbibed by them, frozen in sharp Winters. By which glaciation, the moistened portion of the Soil must forcibly endeavour to expand it self, and actually do so in the parts contiguous to the passage, since there it finds no resistance: And though the expansion made in one year or two be but small, and therefore not observed; yet, in a succession of many Winters, it may by degrees grow to be very considerable. But this suspicion I suggest not, that I would deny the Growth of Minerals, but to recommend this Argument for it to further Consideration.

And

And yet I take this to be a better proof, than what is much relied on by some Writers of Metals, who urge, that in Churches, and other magnificent Buildings, that are Leaded over, the Metalline Roofs, in a long tract of years, grow far more ponderous, in so much that often times there is a necessity to remove them, and exchange them for Brass ones. For though this plausible Argument be urged by several Writers, and among them by the Learned *Jo. Gerhardus, pag. m.* 22, yet I fear they proceed upon a Mistake. For having had some occasion to observe and inquire after this kind of Lead, I soon suspected, that the increment of weight, (which sometimes may indeed be very great) was no clear proof of the real Growth of the Metal it self. For in that which I had occasion to consider, the additional weight as well as bulk seem'd to proceed from Acetous or other Saline Corpuscles of the Timber of those Buildings, which by degrees exhaling

and

and corroding that side of the Lea which they fasten'd on, turned i with themselves into a kind of *Ce russe*: Which suspicion I shall briefl make probable by noting, 1. Tha I have found by tryal purposel made, that Woods afford an acid though not meerly acid, liquor, ca pable of corroding Lead. 2. Tha 'tis known, that Lead turned int Ceruse increases notably in weight some say, (for I had not opportunit to try it) above six or seven in th hundred. 3. That from the Sheet of Lead that have very long cover'd Churches and the like Buildings there is often obtain'd by scraping good proportion of white Lead which I have known much preferr'd by an eminent Artist to common Ce russe, when a white Pigment was t be employed. And, by the way Mens finding this Ceruse not on tha side of the Lead that is expos'd to th outward Air, (where I scarce eve observed any) but on the inside tha regards the Timber and other wooden

work

work, may disabuse those that fancied this Ceruſſe to be a part of the Lead calcin'd by the Beams of the Sun, that ſtrike immediately upon the Metal. And if to this it be added, that by Diſtillation and otherwiſe I have found cauſe to ſuſpect, that *Alabaſter* and *White Marble* may emit Spirituous parts that will invade Lead; it may be doubted, whether what *Galen* relates of the great Intumeſcence of Leaden bands or faſtenings, wherewith the Feet of Statues were faſten'd (to their Pedeſtals,) be a ſure Argument of the real Growth of that Metal in the Air.

But I begin to digreſs, and ſeemingly to the prejudice of the particular Scope of this Paper; but yet not to that of one of the main Scopes of all my *Phyſical* Writings, the Diſquiſition and Advancement of Truth.

O B.

OBSERVATIONS ABOUT THE Growth of IRON.

I Did not find in one of our chief Mines of *Iron*, that there was any notice taken of the Growth of that Metal; but in another place or two some that deal in Iron-Ore, informed me, that they believe it grows, and may be regenerated; and upon that account one of them set up a Work, contiguous to some Land of mine, to melt over again the remainder of Ore that had been already wrought (at a great distance from that place) and had for some Ages lain in heaps exposed to the free Air; but with what success this chargeable Attempt has been made, I am not yet informed.

But

But of the Growth of Iron in the Island of *Ilva* or *Elva*, in the *Tyrrhene Sea*, not far from the Coast of *Tuscany*, not only ancient Authors, as *Pliny* and *Strabo*, take special notice, but modern Mineralists of very good credit, as *Falopius* and *Cæsalpinus*, particularly attest the same thing; of whom the latter speaks thus: *Vena ferri copiosissima est in Italia, ob eam nobilitata; Ilva, Tyrreni Maris Insula, incredibili copiâ etiam nostris temporibus eam gignens: Nam terra, quæ eruitur dum vena effoditur, tota procedente tempore in venam convertitur.* *Lib. III. Cap. 6.*

And the experienc'd *Agricola* gives us the like account of a place in his Country, *Germany*, *In Lygiis*, says he, *ad Sagam oppidum in pratis eruitur ferrum, fossis ad altitudinem bipedaneam actis. Id decennio renatum denuò foditur, non aliter ac Ilvæ ferrum.* Agric. *de Vet. & Nov. Met. Lib. II. Cap. 15.*

The Learned *Johan. Gerhardus*, out of a Book which he calls *Conciones Metallicæ*; I suppose he means the

High-Dutch Sermons of *Matheſius*, (whoſe Language I underſtand not) has this notable paſſage to our preſent purpoſe: *Relatum mihi eſt à metallico foſſore, ad Ferrarias, quæ non longè Ambergâ diſtant, terram inanem cum ferri Minera erutam, quam vocant* **den Gummer**, *mixtam cum recrementis ferri, quæ appellater* **der Sinder**, *congeſtam in cumulos, inſtar magni cujuſdam valli, ſolibus pluviiſque exponi, & decimo quinto anno denuò excoqui, eliquarique ferrum tantæ tenacitatis, ut ſolæ laminæ inde procudantur.*

J. Gerhard. *Profeſſor* Tubingenſis, *Decad. Quæſt. Phyſico-chymicarum, pag. m.* 18.

OB-

OBSERVATIONS
ABOUT THE
Growth of SILVER.

OF the Growth, as is supposed, of *Silver* in the form of Trees or Grass or other Vegetables, I have met with some Instances among Mineralists, and I have elsewhere mention'd, that an Acquaintance of mine shew'd me a Stone, wherein he affirmed the Silver, I saw in it, to have increased since he had it. But for certain Reasons, none of these Relations seem to me very proper to my present purpose; in order to which I shall therefore set down only one Instance, which I lately met with in a *French* Collection of Voyages, publish'd by a Person of great Curiosity and Industry, (from whose

Civility I receiv'd the Book.) For there, in an account given by a Gentleman of his Country of a late Voyage he made to *Peru*, wherein he visited the famous Silver-Mines of *Potosi*, I found a passage which speaks to this sense: *Le meilleur Argent, &c. i. e.* The best Silver in all the *Indies* and the purest is that of the Mines of *Potosi*; the chief have been found in the Mountain of *Aranzasse*: And, (some Lines being interpos'd) 'tis added, that they draw this Metal even from the Mineral Earths that were in times past thrown aside, when the ground was open, and the Groves and Shafts that are in the Mountains were made; it having been observ'd that in these recrements Metal had been formed afresh since those times, which sufficiently shews the propensity of the Soil to the production of this Metal; yet 'tis true, that these impregnated Earths yield not so much as the ordinary Ore which is found in Veins betwixt the Rocks.

Voyage du Sieur au Peru, pag. 15.

OB-

OBSERVATIONS ABOUT THE Growth of GOLD.

AS for the Growth of *Gold*, the Enquiries I have yet made among Travellers give me no great satisfaction about it, and though I have spoken with several that have been at the Coast of *Guiny*, and in *Congo*, and other Parts of *Afric*, where much Gold is to be had; yet I could not learn by them, that they, or any Acquaintance of theirs among the Natives, had seen any Mines or Veins of Gold, (which yet divers Authors affirm to be found in more than one Kingdom in *Ethiopia*, and in some other *African* Countries.) And having afterwards met with a Learned Traveller, that had carefully vi-

sited the famous Gold-Mine of *Cremnitz* in *Hungary*, he answer'd me, That he did not learn from the Miners, whether or no the Ores of Gold, *&c.* did really grow or were regenerated in tract of time, by being expos'd to the Air, or upon any other account; but the Grand Overseer, who was Lord of part of the Soil, told him, that he thought the whole Mountain to abound with particles of Gold, and therefore was wont, when the Diggers had almost exhausted the Vein, to cast-in store of Earth, and dig up other neighbouring places, which, being kept there as in a Conservatory, would afterwards afford Gold, as the Mine did before.

And, if a late *German* Professor of Physic do not misinform us, his Country affords us an eminent Instance of the Growth or Regeneration of Gold.

Johan. Gerhardus *in Decade Quæstionum pag.* m. 19.

Nam Corbachi, says he, *quæ est Civitas Westphaliæ sub ditione Comitis de Isenborg & Waldeck, Aurum excoquitur ex cumulis congestis, ita*

ut

OF THE CAUSE OF Attraction BY SUCTION.

By the Honourable
ROBERT BOYLE,
Fellow of the *Royal Society*.

LONDON,
Printed by *William Godbid*, and are to be Sold by *Moses Pitt*, at the *Angel* over against the little North Door of St. *Paul's* Church. 1674.

OF THE CAUSE OF *ATTRACTION* BY SUCTION.

CHAP. I.

I Might, *Sir*, save my self some trouble in giving you that account you desire of me about *Suction*, by referring you to a passage in the *Examen*, I long since writ, of Mr. *Hobbes*'s *Dialogus Physicus de Natura Aeris*, if I knew, you had those two Books lying by you. But because I suspect, that my *Examen*

may not be in your hands, since 'tis almost out of Print, and has not for some years been in my own; and because I do not so well remember, after so long a time, the particulars that I writ there, about Suction, as I do in general, that the *Hypothesis* I proposed, was very incidentally and briefly discours'd of, upon an occasion ministred by a wrong Explication given of Suction by Mr. *Hobbes*, I shall here decline referring you to what I there writ; and proposing to you those thoughts about Suction, that I remember I there pointed at, I shall annex some things to illustrate and confirm them, that would not have been so proper for me to have insisted on in a short and but occasional Excursion.

And I should immediately proceed to what you expect from me, but that *Suction* being generally look'd upon as a kind of *Attraction*, it will be requisite for me to premise something about *Attraction* it self. For, besides that the Cause of it, which

ut singulis quadriennii iterum elaboretur cumulus unus, semper se restaurante naturâ, &c.

POSTSCRIPT.

SInce the setting down of the foregoing Observations, I casually met with a curious Book of Travels, lately made by the very Ingenious Dr. *Edward Brown*, and finding in *pag.* 100. a couple of Relations, that seem pertinently referable, the one, to a passage above-cited out of *Agricola*, in the Notes about the Growth of Lead, and the other to the present Title about the Growth of Gold; I thought fit to annex them in the Learned Authors own words, *viz.*

1. *Some passages in this Mine cut through the Rock, and long disus'd, have grown up again: And I observed the sides of some, which had been formerly wide enough, to carry their Ore through,*

to approach each other, so as we passed with difficulty. This happens most in moist places; the passages unite not from the top to the bottom, but from one side to another.

2. *The common yellow Earth of the Country near* Cremnitz, *especially of the Hills towards the West, although not esteem'd Ore, affords some Gold: And in one place, I saw a great part of an Hill digg'd away, which hath been cast into the Works, washed and wrought in the same manner as pounded Ore, with considerable profit.*

THe foregoing Observations about the Growth of Gold and other Metals are not all that I might, perhaps without being blamed for it, have referr'd to that Title. But all my Papers, wherein other Observations of this kind were set down, are not now at hand, and divers other Instances, that I have met with among Wri-

Writers of the Growth of Metals, (taking that expreſſion in the ſenſe I formerly declared) do not ſeem to me ſo pertinent in this place, becauſe the improving Ores were not expos'd, nor perchance acceſſible, to the Air. And even as to the Inſtances that I have now mention'd, 'till ſeverer Obſervations have been made, to determin whether it be partly the contact or the operation of the Air, or ſome internal diſpoſition, analogous to a Metalline Seed or Ferment, that cauſes this Metalline Increment, I dare not be poſitive; though I thought the Intereſt of the Air in this Effect might make it pardonable, to add on this occaſion to the Hiſtory of Nature ſome particulars, of which the Cauſe conjecturally propoſed may be probable enough to countenance a Suſpicion, 'till further Experience have more clearly inſtructed us.

To what has been ſaid of the Growth of Metals in the Air, I might add ſome Inſtances of the

Growth

Growth of Fossile Salts, and of some other Minerals: But, besides that these belong to the Paper about the Saltnesses of the Air; what has been already said may suffice for the present occasion.

POSTSCRIPT.

After what I writ in the 23th page of the foregoing Discourse, having an opportunity to look again upon the Marchasite there mention'd to have been Hermetically seal'd up after its surface had been freed from the grains of Vitriolate Salt that adher'd to it, I perceiv'd, that, notwithstanding the Glass had been so closely stopp'd, yet there plainly appear'd from the outside of the mass some grains of an Efflorescence, whose colour, between blew and green, argued it to be of a Vitriolate nature. If this be seconded with other trials made

made with the like ſucceſs, it may ſuggeſt new thoughts about the *Growth of Metals* and Minerals, eſpecially with reference to the Air.

FINIS.

NEW *EXPERIMENTS* About the PRESERVATION OF BODIES IN VACUO BOYLIANO.

By the Honourable
ROBERT BOYLE,
Fellow of the *Royal Society*.

LONDON,
Printed by *William Godbid*, and are to be Sold by *Moses Pitt*, at the *Angel* over against the little North Door of St. *Paul's* Church. 1674.

NEW EXPERIMENTS *ABOUT THE* Preservation of BODIES IN *VACUO BOYLIANO.*

EXPER. I.

A Piece of roasted Rabbet, being exactly clos'd up in an exhausted Receiver the Sixth of *November*, was two months and some few days after taken out without appearing to be corrupted, or sensibly alter'd in Colour, Tast, or Smell.

EXPER. II.

A ſmall Glaſs-Receiver, being half fill'd with pieces of White-bread, (part Cruſt and part Crumb) was exhauſted, and ſecur'd the eleventh of *March:* The Receiver being open'd the firſt of *April*, part of the Bread was ſhaken out, and appear'd not to have been conſiderably, if at all ſenſibly, impair'd in that time, ſave that the outſide of ſome pieces of Crumb ſeem'd to be a little, and but a little, leſs ſoft and white than before. There appear'd no drops or the leaſt Dew on the inſide of the Glaſs. The remaining Bread was again ſecur'd ſoon after.

The eighteenth of *April*, the Bread was taken out again, and taſted much as it did the laſt time, the Cruſt being alſo ſoft, and no drops of water appearing on the inſide of the Glaſs.

EXPER. III.

This day (being the ninth of *March*) I open'd a small exhausted and secur'd Receiver, wherein, about the ninth of *December*, that is, about three months ago, we had included some *Milk*: Upon opening an access to the Air, we found the Milk well colour'd, and turn'd partly into a kind of Whey, and partly into a kind of soft Curd. The taste was not offensive, only a little sowrish like Whey, and the smell was not at all stinking, but somewhat like that of sowrish Milk.

EXPER. IV.

The *Violet-leaves*, that were put up, and freed and secur'd from Air the fifth of *March*, being this day open'd, (*April* the seventh) appear'd not to have chang'd their shape, or colour, or consistence: For, as for their odour, it could not be well judg'd of, because he that included them had, for

his own eaſe, contrary to my expreſs direction, cruſh'd many of them together in thruſting them down; and by ſuch a violation of their Texture, it's natural for Violets to loſe their fragrancy, and acquire an Earthy ſmell.

EXPER. V.

Having carefully placed ſome *Violets* in an exhauſted Receiver, of a convenient ſize and bigneſs, and ſecur'd it from immediate commerce with the external Air; the Seventh month after we look'd upon them again, and found they were not putrified or reſolved into any mucilaginous ſubſtance, but kept their ſhape intire, ſome of them retaining their colour, but more of them having ſo loſt it, as to look like *white* Violets.

EXPER. VI.

November the fifth, we conveyed into a conveniently ſhap'd Receiver

ver some ounces of *Sheeps-blood*, taken from an Animal that had been kill'd that afternoon. And after the exhaustion of the Air, during which, store of bubbles were generated in the Liquor that made it swell notably, the included Blood was kept in a place, (whose warmth we judg'd equal to that of a digestive Furnace) for twenty days; for one or two of the first of which, the Blood seem'd to continue fluid, and of a florid colour, which afterwards degenerated into one that tended more to blackness. On the twenty fifth of *November* we came to let in the external, and found it to rush into the Receiver, and the Glass containing the Blood being held in a lightsom place, the most part of the bottom of it seem'd to be thinly overlaid with a coagulated substance of a higher colour than that which swam above it, which yet, though it appeared dark and almost blackish in the Glass whilst it was look'd on in the bulk, yet, if it was shook, those

parts of it that fell down along the inside of the Glass, appear'd of a deep but fair colour. But whilst the Blood continued in the Glass, it was suppos'd not to stink, since, even when it was poured out, though its smell seem'd to me (whose Organs of Smelling are tender) to have I know not what that was offensive, yet to others it seem'd to smell but as the Blood of a newly kill'd Dog.

EXPER. VII.

Some *Cream* being put up and secur'd the seventeenth of *March* in an exhausted Receiver, did this day appear to be more thick and almost Butter-like at the top (whose *superficies* seem'd rugged) than otherwhere, and afterwards by being well shaken together in the not inconveniently shap'd Glass, was easily enough reduc'd to Butter, whose Butter-milk, by the judgment of those who were more us'd to deal in it than I, appear'd not differing from ordinary Butter-

Butter-milk. And I found it had, like that, a grateful sowrness. The Butter was judg'd to be a little sowrer than ordinary, but was not, as they speak, *made*.

[In the Entry of this Experiment, Blanks were left for the years; but the Tenour of the words, and Design of the Experiment, and other Circumstances, assure me, that the Cream continued a year in the vessel.]

EXPER. VIII.

February the eighteenth we look'd again upon three Vials, that had been exhausted and secur'd the fifteenth of *September* last, the one of these had in it some slices of *roasted Beef*, and the other some shivers of *white Bread*, and the last some thin pieces of *Cheese*; all which seem'd to be free from putrefaction, and look'd much as they did when they were first put up: Wherefore we thought not fit to let the Air into the Receiver, but left them as they were to lengthen the design'd Trial.

EX-

EXPER. IX.

February the eighteenth, there was a fourth Vial, wherein about six months before, *viz. August* the twelfth, had been inclos'd and secur'd some *July-flowers* and a *Rose*; and yet these being kept in the same place with the rest, though they seemed a little moist, retained their shape and colour, especially the *Rose*, which look'd fresh enough to seem to have been gather'd but lately.

N. B. That we observed not in any of these four Receivers any great drops, or so much as Dew in the upper parts, *viz.* those that were situated above the included matter.

EXPER. X.

June the fourth we left some *Strawberries* in an exhausted Receiver, and coming to look upon them after the beginning of *November*, we found them to be discolour'd, but not alter'd in

in shape, nor affording any sign of Corruption by being at all mouldy: Wherefore we thought fit to leave them still in the Receiver for further Trial.

EXPER. XI.

May the second, 1669, a piece of *roasted Beef*, secur'd *September* the fifteenth, appear'd to be not at all alter'd: As did likewise a piece of *Cheese* secur'd in another Receiver; and some pieces of a *French Rose* the same day (*September* the fifteenth) secur'd in a third.

N. B. The Flowers seal'd up *August* the twelfth, 1668, being this day look'd upon, appear'd fresh, and consequently did so after having been kept eight months and an half.

EXPER. XII.

There was taken *Beer* of eight shillings a Barrel, of a year old, near a Pint of which, *June* the seventeenth, was

was put into a conveniently shap'd Glass, and it was afterwards exhausted and secur'd from the Air; the most part of the month of *August* prov'd extraordinarily hot. Towards the latter end there was at several times great Thunder, which made the Beer in our Cellar, and in most of those of the Neighbourhood, turn soure. The first of *September*, the Beer was open'd, but did not seem to have degenerated into any soureness.

EXPER. XIII.

Being desirous to try, whether the *Thunder* would have such effect upon *Ale* exactly stopp'd in Glass-vessels, as it often has on that Liquor in the ordinary wooden Casks; I caus'd some Ale moderately strong to be put into a conveniently shap'd Receiver, and having exhausted the Air and secur'd a Glass-vessel, 'twas put into a quiet, but not cool, place: Last week, which was about six weeks after the Liquor had been inclos'd, there

there happening some very loud Thunder, and our Beer, though the Cask was kept in a good Cellar, being generally noted to have been turn'd soure after this Thunder; I staid yet a day or two longer, that the operation upon our included Liquor might be the more certain and manifest; and then permitting an access to the outward Air, we took out the Ale, and found it to be good drink, and not at all soured.

Compare this with the Wish made in the *Essay* of the *Great Efficacy* of *Effluviums*, *chap.* 5. *pag.* 28. that such an Experiment should be tried.

EXPER. XIV.

September the twenty first, 1670, some *Blackberries*, included in an exhausted Receiver, were open'd *June* the twentieth, 1673, and were found free from all mouldiness and ill sent, only there was found some Liquor that was soure, which being taken out the Berries were secur'd again.

[*At*

[*At the same time was another parce*
of the same Berries *exactly clos'd up i*
a Receiver, whence the Air was no
pump'd, to try what difference in th
Event would appear by this variation
But, coming in October *the eleventh*
1673, *to look upon the Glass, we found*
crack'd, and the Fruit all cover'd ove
with a thick mould. Nor was this th
only Vessel wherein Trials, made to pre
serve Fruits, without any exhaustion
the Air, miscarried.]

October the eleventh, 1674, the sam
Berries, being look'd upon, appear'
to have their colour alter'd, an
much less black than before, but di
not appear putrefied by either loss o
shape, or by any stinking smell, no
was the least mouldiness observe
to be on them; though they had bee
kept in the same Receiver above fou
year.

That *Fructus Horarii*, especially s
tender and juicy ones, should with
out any additament be preserve
from putrefaction so many times lon
ger than otherwise they would hav

lasted

asted, as 'tis more than would be expected, so it may give hopes, hat both odd and useful things of his kind may be this way performed.

POST-

POSTSCRIPT.

THe foregoing Experiments, a the Memorials themſelves de clare, were all of them made *in Vac Boyliano*, nor did I intend to ſet dow any other: But meeting among thoſ Memorials with a ſhort account of couple of Trials made without th help of our Pneumatic Engine, I wa induc'd to annex them, becauſe man may make the like, that will not b able to make ſuch as have been h therto recited. And theſe two r quiring no peculiarly ſhap'd Veſſels 'tis thought, it may prove of ſom Oeconomical as well as Phyſical uſe if it be ſhewn by experience, tha Liquors Hermetically-ſeal'd the or dinary way in common Bolt-head may be kept from ſouring very muc beyond their uſual time of laſting.

Fun

June the fourteenth we put a convenient quantity of good Ale into a Bolt-head, and seal'd it up Hermetically; the next year, on the fifth of *July*, we broke off the Seal, and found the Liquor very good and without any sensible sowreness. The next day it was seal'd up again and set by for *thirteen months*, at which time the neck of the Glass being broken, the Ale was found pretty sowre, and therefore the Trial was prosecuted no farther: So that, though this Liquor would not by this way of Preservation be kept from sowring so long as the Wine, to be mention'd in the following Experiment, yet even a small quantity of it was preserved good at the least above a year, which is very much longer than Ale is wont to keep from sowring.

June the fourteenth, 1670, in a large Bolt-head was Hermetically seal'd up about a Pint, by guess, of *French Claret-wine*, which, when we

came to look upon, *July* the fifth, 1671, appear'd very clear and high colour'd, and had depoſited ſtore of *fetes* at the bottom of the Glaſs, but faſten'd no Tartar that we could perceive to the ſides. Upon the breaking of the ſeal'd end of the Glaſs, the By-ſtanders thought, that there was an eruption of included Air or ſteams, and, above the ſurface of the Wine, there appear'd, to a pretty height, a certain white ſmoak almoſt like a miſt, and then gradually vaniſhed: The Wine continued well-taſted, and was a little rough upon the tongue, but not at all ſowre.

The Bolt-head was ſeal'd up again *July* the ſixth 1671, and ſo ſet by 'till *Auguſt* the fifth 1672, at which time it was open'd again, and then the Wine did ſtill taſt very well.

June the twenty ſixth 1673, the Bolt-head with the ſame Claret-wine was open'd, and was found very good, and was ſeal'd up again.

October the eleventh 1674, the ſame Claret-wine was open'd again, and

in my silence, if Mr. Hobbes *had not as 'twere summon'd me to break it by publishing again his* Explications, *which in my* Examen *of his Dialogue* De Natura Aeris *I had shewn to be erroneous.*

And I did not grow at all more satisfied, to find him so constant as well as stiff an Adversary to interspers'd Vacuities, *by comparing what he maintains in his Dialogue* De Vacuo, *with some things that he teaches, especially concerning God, the Cause of Motion, and the Imperviousness of Glass, in some other of his Writings that are published in the same Volume with it. For since he asserts that there is a God, and owns Him to be the Creator of the World; and since on the other side the Penetration of Dimensions is confessed to be impossible, and he denies that there is any* Vacuum *in the Universe; it seems difficult to conceive, how in a World that is already perfectly full of Bodie, a* Corporeal *Deity, such as he maintains in his* Append. ad Leviath. cap. 3, *can have that access even to the minute parts of the Mundane Matter, that seems requisite to the Attributes and Operations that*

belong to the Deity, in reference to the World. But I leave Divines to consider what Influence the conjunction of Mr. Hobbes's *two Opinions, the Corporeity of the Deity, and the perfect Plenitude of the World, may have on Theology. And perhaps I should not in a Physical Discourse have taken any notice of the proposed Difficulty, but that, to prevent an Imputation on the Study of Natures Works, (as if it taught us rather to degrade than admire their Author,) it seem'd not amiss to hint* (in transitu) *that Mr.* Hobbes's *gross Conception of a Corporeal God, is not only unwarranted by* sound *Philosophy, but ill befriended even by* his own.

My Adversary having propos'd his Problems by way of Dialogue *between* A. *and* B; *'twill not, I presume, be wonder'd at, that I have given the same form to my Animadversions; which come forth no earlier, because I had divers other Treatises, that I was more concern'd for, to publish before them.*

But because it will probably be demanded, why on a Tract that is but short,

my

PREFACE.

UPON *the coming abroad of Mr.* Hobbes's Problemata Physica, *finding them in the hands of an Ingenious Person, that intended to write a Censure of them, which several Employments private and publick have, it seems, hinder'd him to do; I began, as is usual on such occasions, to turn over the leaves of the Book, to see what particular things it treated of. This I had not long done before I found, by obvious passages in the* third *Chapter, or Dialogue, as well as by the Title, which was* Problemata de Vacuo, *that I was particularly concern'd in it; upon which I desired the Possessor of the Book, who readily consented, to leave me to examin that* Dialogue, *on which condition I would leave him to deal with all the rest of the Book. Nor did I look upon the Reflections I meant to make as repugnant*

to the Resolutions I had taken against writing Books of Controversie, since the Explications, Mr. Hobbes *gave of his Problems, seem'd to contain but some Variations of, or an Appendix to, his Tract* De Natura Aeris, *which, being one of the two first pieces that were published against what I had written, was one of those that I had expresly reserv'd my self the liberty to answer. But the* Animadversions *I first made upon Mr.* Hobbes's Problems De Vacuo, *having been casually mislaid e're they were finished; before I had occasion to resume my task, there past time enough to let me perceive, that his Doctrine, which 'twill easily be thought that the* Vacuists *disapproved, was not much relished by most of the* Plenists *themselves, the moderneſt* Peripateticks *and the* Cartesians; *each of them maintaining the* Fullness *of the World, upon their own grounds, which are differing enough from those of our Author, the natural Indisposition I have to Polemical Discourses, easily perswaded me to let alone a Controversie, that did not appear needful: And I had still persisted*

in

Preface to Suction

PREFACE.

HAving about twelve years ago summarily expreſt and publiſh'd my Opinion of the Cauſe of Suction, *and a while before or after brought to the* Royal Society *the Glaſs Inſtrument I employ'd to make it out; I deſiſted for ſome time to add any thing about a Problem, that I had but occaſionally handled: Only, becauſe the Inſtrument I mention'd in my* Examen *of Mr.* Hobbes's *Opinion, and afterwards us'd at* Greſham-College, *was difficult enough to be well made, and not to be procur'd ready made, I did for the ſake of ſome* Virtuoſi, *that were curious of ſuch things, deviſe a ſlight and eaſily made Inſtrument, deſcrib'd in the following Tract, Chap. 4th, in which the chief* Phænomena, *I ſhew'd before the* Society, *were eaſily producible. But afterwards the miſtakes and erroneous Opinions,*

that, in Print as well as in Discourse, I met with, even among Learned Men, about Suction, *and the Curiosity of an Ingenious Person, engaged me to resume that Subject and treat of it, as if I had never before meddled with it, for the reason intimated in the beginning of the insuing Paper. And finding upon the review of my later* Animadversions *on Mr.* Hobbes's Problemata de Vacuo, *that some passages of this Tract are referr'd to there; I saw my self thereby little less than engaged to annex that Discourse to those Animadversions. And this I the rather consented to, because it contains some Experiments, that I have not elsewhere met with, which, together with some other parts of that* Essay, *may, I hope, prove of some use to illustrate and confirm our Doctrine about the Weight and Spring of the Air, and supply the less experienced than ingenious Friends to our Hypothesis with more grounds of answering the later Objections of some Learned Men, against whose endeavours I perceive it will be useful to employ variety of Experiments and other Proofs to*

evince

evince the same Truth; that some or other *of these may meet with those Arguments or evasions with which they strive to elude the force of* the rest.

The Title of the following Essay may sufficiently keep the Reader from expecting to find any other kind of Attraction *discours'd of, than that which is made by* Suction. *But yet thus much I shall here intimate in general, that I have found by Trials purposely made, that the Examples of* Suction *are not the only noted ones of* Attraction, *that may be reduced to* Pulsion.

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iles them) in gene-al, and, which is worſe, to diſparage the making of elaborate Experiments; I judg'd the thing, he ſeem'd to aim at, ſo prejudi-cial to true and uſe-ful Philoſophy, that I thought, it might do ſome ſervice to the leſs knowing, and leſs wary, ſort of Readers, if I tryed to make his own Explications enervate his Authority, and by a ſomewhat particular Examen *of the Solutions he has given of the Problems I am concern'd in, ſhew, that 'tis much more eaſie to undervalue a frequent recourſe to Experiments, than truly to explicate the* Phænomena *of Nature without them. And ſince our Author, ſpeaking of his* Problemata Phyſica, *(which is but a ſmall Book) ſcruples not to tell His Majeſty, to whom he dedicates them, that he has therein compriſed (to ſpeak in his own terms)* the greateſt and moſt probable part

tum antè prodierat Hobbius? Cur non inde potiùs incepiſtis ubi ille deſiit? Cur Principiis ab illo poſitis non eſtis uſi? Cumque Ariſtoteles reſtè dixit, ignorato motu ignorari Naturam, &c.

— Ad Cauſas autem, propter quas proficere ne paululum quidem potuiſtis, nec poteritis, accedunt etiam aliæ, ut odium Hobbii, &c.

part of his Physical Meditations; a
since by the alterations, he has made
what he formerly writ about the Phæn
mena of my Engine, he seems to ha
design'd to give it a more advantageo
form: I conceive, that by these select
Solutions of his, one may, without doi
him the least injustice, make an estima
of his way of discoursing about Natur
things. And though I would not inter
the credit of Experimentarian Philo
phers in no considerabler a Paper tha
this; yet if Mr. Hobbes's Explication
and mine be attentively compared, it wi
not, I hope, by them be found, that th
way of Philosophising he employs, is muc
to be preferr'd before that which he under
values.

ANI-

y Animadversions should take up so much room? It will be requisite, that I ere give an account of the bulk of this reatise.

And first, having found that there as not any one Problem, in whose Explication, as propos'd by Mr. Hobbes, I w cause to acquiesce, I was induc'd for e Readers ease, and that I might be sure do my Adversary no wrong, to transcribe his whole Dialogue, bating some few ransitions, and other Clauses not needful be transferr'd hither.

Next, I was not illing to imitate Mr. obbes, who recites the Dialogue we are nsidering the same xperiments that he d already mentiond in his Tract De atura Aeris, without adding as his own (that I remember) any w one to them. But unwillingness to e the Reader with bare

Credo, (says Mr. Hobbes in his Dialogus Physicus:) Nam 13. motus hic Restitutionis, Hobbii est, & ab illo primo & solo explicatus in Lib. de Corpore, cap. 21. Art. 1. Sine qua Hypothesi, quantuscunque labor, ars, sumptus, ad rerum Naturalium invisibiles causas inveniendas adhibeatur, frustra erit. And speaking of the Gentlemen (to whom it were not here proper for me to give

De Nat. Aeris, p.

give Epithetes) *that us'd to meet at* Gresham-College, *and are known by the Name of the* Royal Society, *he thus treats them and their way of Inquiring into Nature:* Conveniant, studia conferant, Experimenta faciant quantum volunt, nisi & Principiis utantur meis, nihil proficient.

A. Fateris ergo nihil hactenus à Collegis tuis promotam esse scientium Causarum Naturalium, nisi quod Unus eorum Machinam invenerit, quâ motus excitari Aeris possit talis, ut partes Sphæræ simul undiquaque tendant ad Centrum, & ut Hypotheses Hobbianæ, antè quidem satis probabiles, hinc reddantur probabiliores.

B. Nec fateri pudet; nam est aliquid prodire tenus, si non datur ultra.

A. Quid *tenus*? quorsum autem tantus apparatus & sumptus Machinarum factu difficilium, ut eatenus tantum prodiretis quantum

bare Repetitions of t Arguments I emplo in my Examen *of th Tract, invited me endeavour to ma him some amends the exercise of his tience by insertin as occasion was offer five or six new Ex riments, that will perhaps be so ea made by every Rea that will be able (n that I have perspi ously propos'd them) understand them.*

And lastly, si Mr. Hobbes *has been content to m nifie himself and way of treating Physical matters, has been pleas'd speak very slightin ly of Experimentari Philosophers (as*

stil

and appear'd of a good colour, not sowre, but seem'd somewhat less spirituous than other good Claret-wine, perhaps because of the Cold weather.

This, and the foregoing Trial about the Preservation of *Ale*, were made in Mr. *Oldenburg*'s House and Presence.

FINIS.

ANIMADVERSIONS

UPON

Mr. HOBBES's *PROBLEMATA*

DE

VACUO.

By the Honourable

ROBERT BOYLE,

Fellow of the *Royal Society*.

LONDON,

Printed by *William Godbid*, and are to be Sold by *Moses Pitt*, at the *Angel* over against the little North Door of St. *Paul's* Church. 1674.

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ANIMADVERSIONS UPON Mr. HOBBES's *Problemata de* Vacuo.

A. MAy one, without too bold an inquisitiveness, ask, what Book you are reading so attentively?

B. You will easily believe you may, when I shall have answer'd you, that 'twas Mr. *Hobbes*'s lately publish'd Tract of Physical Problems, which I was perusing.

A. What progress have you made in it?

B. I was finishing the third Dialogue or Chapter when you came in,

and finding my ſelf, though not named, yet particularly concern'd, I was peruſing it with that attention which it ſeems you took notice of.

A. Divers of your Experiments are ſo expreſly mention'd there, that one need not be skill'd in decyphering to perceive that you are intereſſed in that Chapter, and therefore ſeeing you have heedfully read it over, pray give me leave to ask your Judgment, both of Mr. *Hobbes*'s Opinion, and his Reaſonings about *Vacuum*.

B. Concerning his Opinion, I am ſorry I cannot now ſatisfie your Curioſity, having long ſince taken, and ever ſince kept, a Reſolution to decline, at leaſt until a time that is not yet come, the declaring my ſelf either for or againſt the *Pleniſts*. But as to the other part of your Queſtion, which is about Mr. *Hobbes*'s Arguments for the abſolute Plenitude of the World, I ſhall not ſcruple readily to anſwer, that his Ratiocinations ſeem to me far ſhort of that cogency, which the noiſe he would make in

the

the world, and the way wherein he treats both ancient and modern Philosophers that dissent from him, may warrant us to expect.

A. You will allow me the freedom to tell you, That, to convince me, that your resentment of his explicating divers of the Phænomena of your Pneumatic Engine otherwise than you have been wont to do, (and perhaps in terms that might well have been more civil,) has had no share in dictating this Judgment of yours; the best way will be, that entering for a while into the party of the *Vacuists* you answer the Arguments he alledges in this Chapter to confute them.

B. Having always, as you know, forborn to declare my self either way in this Controversie, I shall not tye my self strictly to the Principles and Notions of the *Vacuists*, nor, though but for a while, oppose my self to those of the *Plenists*: But so far I shall comply with your Commands, as either upon the Doctrine of the *Vacuists*, or upon other grounds, to consider, *whe-*

ther this Dialogue of Mr. *Hobbes* have cogently proved his, and the Schools Assertion, *Non dari Vacuum*; and *whether* he has rightly explain'd some Phænomena of Nature which he undertakes to give an account of, and especially some produced in our Engin whereof he takes upon him to render the genuine Causes. And this last inquiry is that which I chiefly design.

A. By this I perceive, that if you can make out your own Explications of your Adversaries Problems *de Vacuo*, and shew them to be preferable to his, you will think you have done your work, and that 'tis but your secondary scope to shew, that in Mr. *Hobbes* his way of solving them, he gives the *Vacuists* an advantage against *Him*, though not against the *Plenists* in general.

B. You do not mistake my meaning, and therefore without any further Preamble, let us now proceed to the particular Phænomena consider'd by Mr. *Hobbes*; the first of which is an Experiment proposed by me in the

one

one and thirtieth of the *Physico-Mechanical Experiments* concerning the Adhesion of two flat and polish'd Marbles, which I endeavour'd to solve by the pressure of the Air. And this Experiment Mr. *Hobbes* thinks so convincing an one to prove the Plenitude of the World, that, though he tells us he has many cogent Arguments to make it out, yet he mentions but this one, because that, *he says*, suffices.

A. The Confidence he thereby expresses of the great force of this Argument does the less move me, because, I remember, that formerly in his *Elements of Philosophy* he thought it sufficient to employ one Argument to evince the Plenitude of the World, and for that one he pitch'd upon the Vulgar Experiment of a Gardeners Watering-Pot: But, whether he were wrought upon by the Objections made to his Inference from that Phænomenon in your Examen of his Dialogue *De Natura Aeris*, or by some other Considerations, I will not pretend to divine. But I plainly perceive, he

now prefers the Experiment of the cohering Marbles.

B. Of which it will not be amiss, though the passage be somewhat long, to read you his whole Discourse out of the Book I have in my hand.

A. 'Tis fit that you, who for my sake are content to take the pains of answering what he says, should be eased of the trouble of reading it, which I will therefore, with your leave, take upon me. His Discourse then about the Marbles is this:

A. *Ad probandam Universi Plenitudinem, nullum nostin' Argumentum cogens?*

B. *Imò multa: Unum autem sufficit ex eo sumptum, Quod duo corpora plana, si se mutuò secundùm amborum planitiem communem tangant, non facile in instante divelli possunt; successivè verò facillimè. Non dico, impossibile esse duo durissima Marmora ita cohærentia divellere, sed difficile; & vim postulare tantam, quanta sufficit ad duritiem lapidis superandam. Siquidem verò majore vi ad separationem opus sit quàm illa, quâ moven-*

moventur separata, id signum est non dari Vacuum.

A. Assertiones illæ demonstratione indigent. Primò autem ostende, quomodo ex duorum durissimorum corporum, conjunctorum ad superficies exquisite læves, diremptione difficili, sequatur Plenitudo Mundi?

B. Si duo plana, dura, polita Corpora (ut Marmora) collocentur unum supra alterum, ita ut eorum superficies se mutuò per omnia puncta exactè, quantum fieri potest, contingant, illa sine magna difficultate ita divelli non possunt, ut eodem instante per omnia puncta dirimantur. Veruntamen Marmora eadem, si communis eorum superficies ad Horizontem erigatur, aut non valde inclinetur, alterum ab altero facillimè (ut scis) etiam solo pondere dilabentur. Nonne causa hujus rei hæc est, Quod labenti Marmori succedit Aer, & relictum locum semper implet?

A. Certissimè. Quid ergo?

B. Quando verò eadem uno instante divellere conaris, nonne multo major vis adhibenda est; Quam ob causam?

A. Ego,

A. *Ego, & mecum (puto) omnes eau*
ſam ſtatuunt, Quod ſpatium totum inte
duo illa Marmora divulſa, ſimul uno in
ſtante implere Aer non poteſt, quanta
cunque celeritate fiat divulſio.

B. *An qui ſpatia in Aere dari va*
cua contendunt, in illo Aere ſolo dar
negant qui Marmora illa conjuncta cir
cumdat?

A. *Minimè, ſed ubique interſperſa.*

B. *Dum ergo illi, qui Marmor unum*
ab altero revellentes Aerem comprimunt
& per conſequens Vacuum exprimunt
Vacuum faciunt locum per revulſionem
relictum; nulla ergo ſeparationis eri
difficultas, ſaltem non major quàm eſt
difficultas corpora eadem movendi in
Aere poſtquam ſeparata fuerint. Ita
que quoniam, conceſſo Vacuo, difficultas
Marmora illa dirimendi nulla eſt, ſe
quitur per difficultatis experientiam,
nullum eſſe Vacuum.

A. *Recte quidem illud infers. Mun*
di autem Plenitudine ſuppoſita, quomodo
demonſtrabis poſſibile omnino eſſe ut di
vellantur?

B. *Cogita primo Corpus aliquod du*
ctile,

ctile, nec nimis durum, ut ceram, in duas partes distrahi, quæ tamen partes non minus exactè in communi plano se mutuo tangunt quàm lævissima Marmora. Jam quo pacto distrahatur cera, consideremus. Nonne perpetuo attenuatur donec in filum evadat tenuissimum, & omni dato crasso tenuius, & sic tandem divellitur? Eodem modo etiam durissima columna in duas partes distrahetur, si vim tantam adhibeas, quanta sufficit ad resistentiam duritiei superandam. Sicut enim in cera partes primò extimæ distrahuntur, in quarum locum succedit Aer; ita etiam in Corpore quantumlibet duro Aer locum subit partium extimarum, quæ primæ Vulsionis viribus dirumpuntur. Vis autem quæ superat resistentiam partium extimarum Duri, facilè superabit resistentiam reliquarum. Nam resistentia prima est à Toto Duro, reliquarum verò semper à Residuo.

A. *Ita quidem videtur consideranti, quàm Corpora quædam, præsertim verò durissima, fragilia sint.*

Does this Ratiocination seem to you as cogent, as it did to the Proposer of it?

B. You

B. You will quickly think it does not, and perhaps you will think it should not, if you please to consider with me some of the Reflections that the Reading of it suggested to me.

And first, without declaring for the *Vacuists* Opinion, I must profess my self unsatisfied with Mr. *Hobbes's* way of arguing against them: For where he says, *Dum ergo illi qui Marmor unum ab altero revellentes Aerem comprimunt & per consequens Vacuum exprimunt, Vacuum faciunt locum per revulsionem relictum; nulla ergo separationis erit difficultas, saltem non major quàm est difficultas corpora eadem movendi in Aere postquam separata fuerint. Itaque quoniam, concesso Vacuo, difficultas Marmora illa dirimendi nulla est, sequitur per difficultatis experientiam, nullum esse Vacuum.* Methinks he expresses himself but obscurely, and leaves his Readers to ghess, what the word *Dum* refers to. But that which seems to be his drift in this passage, is, that, since the *Vacuists* allow interspersed Vacuities, not only in the Air

that

that ſurrounds the conjoyned Marbles; but in the reſt of the ambient Air, there is no reaſon, why there ſhould be any difficulty in ſeparating the Marbles, or at leaſt any greater difficulty than in moving the Marbles in that Air after their ſeparation. But, not to conſider, whether his Adverſaries will not accuſe his phraſe of *ſqueezing out a Vacuum* as if it were a Body, they will eaſily anſwer, that notwithſtanding the Vacuities they admit in the ambient Air, a manifeſt reaſon may be given in their Hypotheſis of our finding a difficulty in the Divulſion of the Marbles. For, the Vacuities they admit being but interſpers'd, and very ſmall, and the Corpuſcles of the Atmoſphere being according to them endow'd with Gravity, there leans ſo many upon the upper ſurface of the uppermoſt Marble, that that ſtone cannot be at once perpendicularly drawn up from the lower Marble contiguous to it; without a force capable to ſurmount the weight of the Aerial Corpuſcles

that

that lean upon it. And this weight has already so constipated the neighbouring parts of the ambient Air that he, that would perpendicularly raise the upper Marble from the lower, shall need a considerable force to make the Revulsion, and compel the already contiguous parts of the incumbent Air to a subingression into the pores or intervals intercepted between them. For the *Conatus* of him that endeavours to remove the upper Marble, whilst the lower surface of it is fenc'd from the pressure of the Atmosphere by the Contact of the lower Marble which suffers no Air to come in between them, is not assisted by the weight or pressure of the Atmosphere, which, when the Marbles are once separated, pressing as strongly against the undermost surface of the upper Marble, as the incumbent Atmospherical Pillar does against the upper surface of the same Marble, the hand that endeavours to raise it in the free Air has no other resistance, than that small one of the Marbles own weight to surmount. *A.* But

A. But what say you to the Reason that Mr. *Hobbes*, and, as he thinks, all others give of the difficulty of the often mention'd Divulsion, namely, *Quòd spatium totum inter duo illa Marmora divulsa simul uno instante implere Aer non potest, quantacunque celeritate fiat divulsio.*

B. I say, that, for ought I know, the Plenists may give a more plausible account of this Experiment, than Mr. *Hobbes* has here done; and therefore abstracting from the two opposite Hypotheses, I shall further say, That the genuine Cause of the Phænomenon seems to be that which I have already assign'd; and that difficulty of raising the upper stone that accompanies the Airs not being able to come in all at once, to possess the space left between the surfaces of the two Marbles upon their separation, proceeds from hence, that, 'till that space be fill'd with the Atmospherical Air, the hand of him that would lift up the superiour Marble cannot be fully assisted by the pressure of the

the Air against the lower surface c
that Marble.

A. This is a Paradox, and there
fore I shall desire to know on wha
you ground it?

B. Though I mention it but as
Conjecture propos'd *ex abundanti*
yet I shall on this occasion counte
nance it with two things; the *first*
that, since I declare not for the Hypo
thesis of the *Plenists* as 'tis maintain'
by Mr. *Hobbes*, I am not bound to al
low, what the common Explication
adopted by my Adversary, suppose
namely, that either Nature abhors
Vacuum (as the Schools would hav
it,) or that there could be no Divul
sion of the Marbles, unless at the sam
time the Air were admitted into th
room that Divulsion makes for it
And a *Vacuist* may tell you, that
provided the strength employ'd t
draw up the superiour Marble b
great enough to surmount the weigh
of the Aerial Corpuscles accumulated
upon it, the divulsion would ensue
though by Divine Omnipotence no

Air

Air or other Body ſhould be permitted to fill the room made for it by the divulſion; and that the Air's ruſhing into that ſpace does not neceſſarily accompany, but in order of Nature and time follow upon, a ſeparation of the Marbles, the Air that ſurrounded their contiguous ſurfaces being by the weight of the collaterally ſuperiour Air impell'd into the room newly made by the divulſion. But I ſhall rather countenance what you call my Paradox by an Experiment I purpoſely made in our Pneumatical Receiver, where having accommodated two flat and poliſh'd Marbles, ſo that the lower being fixt, the upper might be laid upon it and drawn up again as there ſhould be occaſion, I found, that if, when the Receiver was well exhauſted, the upper Marble was by a certain contrivance laid flat upon the lower, they would not then cohere as formerly, but be with great eaſe ſeparated, though it did not by any Phænomenon appear, that any Air could

come to rush in, to possess the place given it by the recess of the upper Marble, whose very easie avulsion is as easily explicable by our Hypothesis; since the pressure of that little Air, that remain'd in the Receiver, being too faint to make any at all considerable resistance to the avulsion of the upper Marble, the hand that drew it up had very little more than the single weight of the stone to surmount.

A. An *Anti-pleniſt* had expected, that you would have observed, that the difficult separation of the Marbles in the open Air does rather prove, that there may be a *Vacuum*, than that there can be none. For in case the Air can succeed as fast at the sides as the divulsion is made, a *Vacuiſt* may demand, whence comes the difficulty of the separation? And if the Air cannot fill the whole room made for it by the separated Marbles at the same instant they are forc'd asunder, how is a *Vacuum* avoided for that time, how small soever, that is necessary

ceſſary for the Air to paſs from the edges to the middle of the room newly made?

B. What the *Pleniſts* will ſay to your Argument I leave them to conſider; but I preſume, they will be able to give a more plauſible account of the Phænomenon we are treating of, than is given by Mr. *Hobbes.*

A. What induces you to diſlike his Explication of it?

B. Two things; the one, that I think the Cauſe he aſſigns improbable; and the other, that I think another, that is better, has been aſſign'd already.

And *firſt*, whereas Mr. *Hobbes* requires to the Divulſion of the Marbles a force great enough to ſurmount the hardneſs of the ſtone, this is aſſerted *gratis*, which it ſhould not be; ſince it ſeems very unlikely, that the weight of ſo few pounds as will ſuffice to ſeparate two coherent Marbles of about an Inch, for inſtance, in Diameter, ſhould be able to ſurmount the hardneſs of ſuch ſolid

ſtones as we uſually employ in this Experiment. And though it be generally judg'd more eaſie to bend, if it may be, or break a broader piece of Marble *cæteris paribus*, than a much narrower; yet, whereas neither I, nor any elſe that I know, nor I believe Mr. *Hobbes*, ever obſerv'd any difference in the reſiſtance of Marbles to ſeparation from the greater or leſſer thickneſs of the ſtones; I find by conſtant experience, that, *cæteris paribus*, the broadneſs of the coherent Marbles does *exceedingly* increaſe the difficulty of disjoyning them: Inſomuch that, whereas not many pounds, as I was ſaying, would ſeparate Marbles of an Inch, or a leſſer, Diameter; when I increaſed their Diameter to about four Inches, if I miſremember not, there were ſeveral Men that ſucceſſively try'd to pull them aſunder without being able by their utmoſt force to effect it.

A. But what ſay you to the Illuſtration, that Mr. *Hobbes*, upon the ſuppoſition of the Worlds Plenitude, gives

gives of our Phænomenon by drawing asunder the opposite parts of a piece of Wax?

B. To me it seems an Instance improper enough. For *first*, the parts that are to be divided in the Wax are of a soft and yielding consistence, and according to him of a ductile, or, if you please, of a tractile nature, and not, as the parts of the coherent Marbles, very solid and hard. *Next*, the parts of the Wax do not stick together barely by a superficial contact of two smooth Planes, as do the Marbles we are speaking of; but have their parts implicated, and as it were intangled with one another. And therefore they are far from a disposition to slide off, like the Marbles, from one another, in how commodious a posture soever you place them. Besides 'tis manifest, that the Air has opportunity to succeed in the places successively deserted by the receding parts of the attenuated Wax; but 'tis neither manifest, nor as yet well proved by Mr. *Hobbes*, that the

Air does after the same manner succeed between the two Marbles, which, as I lately noted, are not forced asunder after such a way, but are, as himself speaks, sever'd in all their points at the same instant.

A. I know, you forget not what he says of the dividing of a hard Column into two parts by a force sufficient to overcome the resistance of its hardness.

B. He does not here either affirm, that he, or any he can trust, has seen the thing done; nor does he give us any such account of the way wherein the Pillar is to be broken, whether in an erected, inclined, or horizontal posture; nor describe the particular circumstances that were fit to be mention'd in order to the solution of the Phænomenon. Wherefore, 'till I be better inform'd of the matter of fact, I can scarce look upon what Mr. *Hobbes* says of the Pillar, as other than his Conjecture, which now I shall the rather pass by, not only because the case is differing

fering from that of our poliſh'd Marbles, which are actually diſtinct Bodies, and only contiguous in one Commiſſure; but alſo, becauſe I would haſten to the *ſecond* reaſon of my diſlike of Mr. *Hobbes*'s Explication of our Phænomenon, which is, that a better has been given already, from the preſſure of the Atmoſphere upon all the ſuperficial parts of the upper Marble ſave thoſe that touch the Plane of the lower.

A. You would have put fair for convincing Mr. *Hobbes* himſelf, at leaſt would have put him to unuſual ſhifts, if you had ſucceeded in the attempt you made, among other of your Phyſico-Mechanical Experiments, to disjoyn two coherent Marbles, by ſuſpending them horizontally in your Pneumatical Receiver, and pumping out the Air that inviron'd them; for, from your failing in that attempt, though you rendred a not improbable Reaſon of it, Mr. *Hobbes* took occaſion, in his Dialogue *De Natura Aeris*, to ſpeak in

so high a strain as this: *Nihil isthic erat quod ageret pondus; Experimento hoc excogitari contra opinionem eorum qui Vacuum asserunt aliud argumentum fortius aut evidentius non potuit. Nam si duorum cohærentium alterutrum secundùm eam viam, in qua jacent ipsæ contiguæ superficies, propulsum esset, facile separarentur, Aere praximo in locum relictum successivè semper influente; sed illa ita divellere, ut simul totum amitterent contactum, impossibile est, mundo pleno. Oporteret enim aut motum fieri ab uno termino ad alium in instante, aut duo corpora eodem tempore in eodem esse loco: Quorum utrumvis dicere, est absurdum.*

B. You may remember, that where I relate that Experiment, I expres'd a hope, that, when I should be better accommodated than I then was, I might attempt the Tryal with prosperous success, and accordingly afterwards, having got a lesser Engine than that I used before, wherewith the Air might be better pumpt out and longer kept out, I cheerfully repeated the Tryal. To shew then,

that

that when two coherent Marbles are sustained horizontally in the Air, the Cause, why they are not to be forc'd asunder, if they have two or three Inches in Diameter, without the help of a considerable weight, is the pressure I was lately mentioning of the ambient Air; I caused two such coherent Marbles to be suspended in a large Receiver, with a weight at the lowermost, that might help to keep them steddy, but was very inconsiderable to that which their Cohesion might have surmounted; then causing the Air to be pumpt by degrees out of the Receiver, for a good while the Marbles stuck close together, because during that time the Air could not be so far pumpt out, but that there remained enough to sustain the small weight that endeavoured their divulsion: But when the Air was further pumpt out, at length the Spring of the little, but not a little expanded, Air, that remained, being grown too weak to sustain the lower Marble

and

and its small clog, they did, as I expected, drop off.

A. This will not agree over-well with the confident and triumphant expressions just now recited.

B. I never envied Mr. *Hobbes*'s forwardness to triumph, and am content, his Conjectures be recommended by the confidence that accompanies them, if mine be by the success that follows them. But to confirm the Explication given by me of our Phænomenon, I shall add, that as the last mention'd Tryal, which I had several times occasion to repeat, shews, that the cohesion of our two contiguous Marbles would cease upon the withdrawing of the pressure of the Atmosphere; so by another Experiment I made, it appears, that the supervening of that pressure sufficed to cause that Cohesion. For, in prosecution of one of the lately mentioned Tryals, having found, that when the Receiver was well exhausted, two Marbles, though considerably broad, being laid upon one ano-

another after the requisite manner, their adhesion was, if any at all, so weak, that the uppermost would be easily drawn up from off the other; we laid them again one upon the other, and then letting the external Air flow into the Receiver, we found, according to expectation, that the Marbles now cohered well, and we could not raise the uppermost but accompanied with the lowermost. But I am sensible, I have detained you too long upon the single Experiment of the Marbles: And though I hope the stress Mr. *Hobbes* lays on it will plead my excuse, yet to make your Patience some amends, I shall be the more brief in the other particulars that remain to be consider'd in his Dialogue *De Vacuo.* And 'twill not be difficult for me to keep my promise without injuring my Cause, since almost all these particulars being but the same which he has already alledged in his Dialogue *De Naturâ Aeris*, and I soon after answered in my *Examen* of that Dialogue,

logue, I shall need but to refer you to the passages where you may find these Allegations examin'd, only subjoyning here some Reflections upon those few and slight things, that he has added in his Problems *De Vacuo.*

A. I may then, I suppose, read to you the next passage to that long one, you have hitherto been considering, and it is this: *Ad Vacuum nunc revertor: Quas causas sine suppositione Vacui redditurus es illorum effectuum, qui ostenduntur per Machinam illam quæ est in Collegio* Gresham*ensi?*

B. *Machina illa* —

B. Stop here, I beseech you, a little, that, before we go any further, I may take notice to you of a couple of things that will concern our subsequent Discourse.

Whereof the *first* is, that it appears by Mr. *Hobbes's Dialogue about the Air*, that the Explications he there gave of some of the Phænomena of the *Machina Boyliana*, were directed partly against the *Virtuosi*, that have since been honour'd with the Title of

of the *Royal Society*, and partly against the Author of that Engine, as if the main thing therein design'd were to prove a *Vacuum*. And since he now repeats the same explications, I think it necessary to say again, that if he either takes the *Society* or me for profes's'd *Vacuists*, he mistakes, and shoots beside the mark; for, neither they nor I have ever yet declar'd either *for* or *against* a *Vacuum*.

And the *other* thing I would observe to you, is, that Mr. *Hobbes* seems not to have rightly understood, or at least not to have sufficiently heeded in what chiefly consists the advantage, which the *Vacuists* may make of our Engine against him: For, whereas in divers places he is very solicitous to prove, that the cavity of our Pneumatical Receiver is not altogether empty, the Vacuists may tell him, that since he asserts the *absolute* plenitude of the World, he must, as indeed he does, reject not only great Vacuities, but also those very small and interspers'd ones,

ones, that they suppose to be inter-
cepted between the solid corpuscle
of other bodies, particularly of th
Air: So that it would not confut
them to prove, that in our Receiver
when most diligently exhausted
there is not one great and absolut
Vacuity, or, as they speak, a *Vacuum coacervatum*, since smaller and disse
minated Vacuities would serve thei
turn. And therefore they may thinl
their Pretensions highly favour'd, a
by several particular effects, so by
this general Phænomenon of our En-
gine, that it appears by several Cir
cumstances, that the Common or At
mospherical Air, which, before
the pump is set a work, possess'd
the whole cavity of our Receiver
far the greatest part is by the inter-
vention of the pump made to pass
out of the cavity into the open Air
without being able, at least for a
little while, to get in again; and
yet it does not appear by any thing
alledg'd by Mr. *Hobbes*, that any o-
ther body succeeds to fill adequately
the

the places deserted by such a multitude of Aerial corpuscles.

A. If I ghess aright, by those words, (viz. *it appears not by any thing alledg'd by Mr.* Hobbes,) you design to intimate, that you would not in general prejudice the Plenists.

B. Your conjecture was well founded: For I think divers of them, and particularly the *Cartesians*, who suppose a subtile Matter or *Æther* fine enough to permeate glass, though our common Air cannot do it, have not near so difficult a task to avoid the Arguments the Vacuists may draw from our Engine, as Mr. *Hobbes*, who, without having recourse to the porosity of glass, which indeed is impervious to *common* Air, strives to solve the Phænomena, and prove our Receiver to be always perfectly full, and therefore as full at any one time as at any other of common or Atmospherical Air, as far as we can judge of his opinion by the tendency or import of his Explications.

A. Yet, if I were rightly inform'd of

of an Experiment of yours, Mr. *Hobbes* may be thereby reduc'd either to pass over to the *Vacuists*, or to acknowledge some Ætherial or other matter more subtil than Air, and capable of passing through the pores of glass; and therefore, to shew yourself impartial between the Vacuists and their Adversaries in this Controversie, I hope you will not refuse to gratifie the *Plenists* by giving your friends a more particular account of the Experiment.

B. I know which you mean, and remember it very well. For, though I long since devis'd it, yet having but the other day had occasion to peruse the Relation I writ down of one of the best Tryals, I think I can repeat it, almost in the very words, which, if I mistake not, were these:

There was taken a Bubble of thin white glass, about the bigness of a Nutmeg, with a very slender stem, of about four or five Inches long, and of the bigness of a Crows-quill.

The

The end of the Quill being held in the flame of a Lamp blown with a pair of Bellows, was readily and well seal'd up, and presently the globous part of the glass, being held by the stem, was kept turning in the flame, 'till it was red hot and ready to melt; then being a little removed from the flame, as the included Air began to lose of its agitation and spring, the external Air manifestly and considerably press'd in one of the sides of the Bubble. But the glass being again, before the cold could crack it, held as before in the flame, the rarified Air distended and plump'd up the Bubble; which being the second time remov'd from the flame, was the second time compress'd; and, being the third time brought back to the flame, swell'd as before, and remov'd, was again compress'd, (either this time or the last by two distinct cavities;) 'till at length, having satisfied our selves, that the included Air was capable of being condens'd or dilated without the ingress or

egress of Air (properly so called) we held the Bubble so long in the flame, strengthen'd by nimble blasts, that not only it had its sides plump'd up, but a hole violently broken in it by the over-rarified Air, which, together with the former watchfulness, we imploy'd from time to time to discern if it were any where crackt or perforated, satisfied us that it was till then intire.

A. I confess, I did not readily conceive before, how you could, (as I was told you had,) make a solid Vessel, wherein there was no danger of the Aires getting in or out, whose cavity should be still possest with the same Air, and yet the Vessel be made by turns bigger and lesser. And, though I presently thought upon a well stopt bladder, yet I well foresaw, that a distrustful Adversary might make some Objections, which are by your way of proceeding obviated, and the Experiment agrees with your Doctrine in shewing, how impervious we may well think your thick

Pneu-

Pneumatick Receivers are to common Air, since a thin glass Bubble, when its pores were open'd or relax'd by flame, would not give passage to the Springy particles of the Air, though violently agitated; for if those particles could have got out of the pores, they never would have broke the Bubble, as at length a more violent degree of Heat made them do; nor probably would the Compression, that afterwards insued of the Bubble by the ambient Air, be check't near so soon, if those Springy Corpuscles had not remained within to make the resistance. Methinks, one may hence draw a new proof of what I remember you elsewhere teach, that the Spring of the Air may be much strengthen'd by Heat. For, in our case, the Spring of the Air was thereby inabled to expand the compress glass, it was impriſon'd in, in spite of the resisting pressure of the external Air; and yet, that this pressure was considerable, appears by this, that the weight of so small a Column

of Atmospherical Air, as could bear upon the Bubble, was able to press in the heated glass, in spite of the resistance of its tenacity and arched figure.

B. Yet that which I mainly design'd in this Experiment was, (if I were able) to shew and prove at once, by an Instance not lyable to the ordinary exceptions, the true Nature of Rarefaction and Condensation, at least of the Air. For, to say nothing of the *Peripatetick* Rarefaction and Condensation, strictly so call'd, which I scruple not to declare, I think to be physically inconceptible or impossible; 'tis plain by our Experiment, that, when the Bubble, after the Glass had been first thrust in towards the Center, was expanded again by heat, the included Air *possess'd* more room than before, and yet it could perfectly *fill* no more room than formerly, each Aerial Particle taking up, both before and after the heating of the Bubble, a portion of space adequate to its own bulk; so that in the Cavity of the expan-

expanded Bubble we must admit either Vacuities interspers'd between the Corpuscles of the Air, or that some fine Particles of the Flame, or other subtil matter, came in to fill up those Intervals, which matter must have enter'd the Cavity of the Glass at its pores: And afterwards, when the red-hot Bubble was removed from the flame, it is evident, that, since the grosser particles of the Air could not get through the Glass, which they were not able to do, even when vehemently agitated by an ambient Flame, the Compression of the Bubble, and the Condensation of the Air, which was necessarily consequent upon it, could not, supposing the Plenitude of the World, be performed without squeezing out some of the subtil matter contained in the cavity of the Bubble, whence it could not issue but at the pores of the Glass. But I will no longer detain you from Mr. *Hobbes* his Explications of the *Machina Boyliana*; to the first of which you may now, if you please, advance.

A. The passage I was going to read, when you interrupted me, was this:

B. *Machina illa eosdem effectus producit, quos produceret in loco non magno magnus inclusus ventus.*

A. *Quomodò ingreditur istuc ventus? Machinam nosti Cylindrum esse cavum, æneum, in quem protruditur Cylindrus alius solidus ligneus, corio tectus, (quem suctorem dicunt) ita exquisitè congruens, ut ne minimus quidem Acr inter corium & æs intrare (ut putant) possit.*

B. *Scio, & quò Suctor facilius intrudi possit, foramen quoddam est in superiori parte Cylindri, per quod Aer (qui suctoris ingressum alioqui impedire possit) emittatur. Quod foramen aperire possunt & claudere quoties usus postulat. Est etiam in Cylindri cavi recessu summo datus aditus Aeri in globum concavum Vitreum, quem etiam aditum claviculâ obturare & aperire possunt quoties volunt. Denique in globo vitreo summo relinquitur foramen satis amplum, (claviculâ item claudendum & recludendum) ut in illum quæ volunt immittere possint, experiendi causâ.* B. The

B. The imaginary wind to which Mr. *Hobbes* here ascribes the effects of our Engine, he formerly had recourse to in the 13th page of his *Dialogue*, and I have sufficiently answer'd that passage of it in the 45th and 46th pages of my *Examen*, to which I therefore refer you.

A. I presume, you did not overlook the comparison Mr. *Hobbes* annexes to what I last read out of his Problems, since he liked the conceit so well, that we meet with it in this place again, though he had formerly printed it in his Dialogue *De Natura Aeris.* The words (as you see) are these: *Tota denique Machina non multum differt, si naturam ejus spectes, à Sclopeto ex Sambuco, quo pueri se delectant, imitantes Sclopetos militum, nisi quòd major sit, & majori arte fabricatus, & pluris constet.*

B. I could scarce, for the reason you give, avoid taking notice of it. And if Mr. *Hobbes* intended it for a piece of Ralliery, I willingly let it pass, and could easily forgive him a more

considerable attempt than this, to be reveng'd on an Engine that has destroyed several of his opinions: But, if he seriously meant to make a Physical Comparison, I think he made a very improper one. For, not to urge, that one may well doubt how he knows, that in the incloſed cavity of his Pot-gun, there is a very vehement wind, (since that does not necessarily follow from the compreſſion of the included Air:) In Mr. *Hobbes*'s Instrument, the Air, being forcibly compreſt, has an endeavour to expand it self, and when it is able to surmount the reſiſtance of its priſon, that part that is first disjoyn'd is forcibly thrown outwards; whereas in our Engine it appears by the paſſage lately cited of our *Examen*, that the Air is not compreſt but expanded in our Receiver, and if an intercourſe be open'd, or the Veſſel be not strong enough, the outward Air violently ruſhes in: And if the Receiver chance to break, the fragments of the glaſs are not thrown outwards, but forced inwards.

A. So

A. So that, whether or no Mr. *Hobbes* could have pitch'd upon a Compariſon more ſuitable to his Intentions, he might eaſily have imployed one more ſuitable to the *Phænomena.*

B. I preſume, you will judge it the leſs agreeable to the Phænomena, if I here ſubjoyn an Experiment, that poſſibly you will not diſlike; which I devis'd to ſhew, not only that in our exhauſted Receivers there is no ſuch ſtrong endeavour outwards, as moſt of Mr. *Hobbes*'s Explications of the things that happen in them are built upon, but that the weight of the Atmoſpherical Air, when 'tis not reſiſted by the counterpreſſure of any internal Air, is able to perform what a weight of many pounds would not ſuffice to do.

A. I ſhall the more willingly learn an Experiment to this purpoſe, becauſe in your Receivers, the rigidity of the glaſs keeps us from ſeeing, by any manifeſt change of its figure, whether, if it could yield without breaking,

breaking, it would be pres's'd in, as your Hypothesis requires.

B. The desires to obviate that very difficulty, for their satisfaction, that had not yet penetrated the grounds of our *Hypothesis*, made me think of employing, instead of a Receiver of Glass, one of a stiff and tough, but yet somewhat flexible Metal. And accordingly having provided a new Pewter Porrenger, and whelm'd it upside down upon an Iron plate fasten'd to (the upper end of) our Pneumatical Pump, we carefully fasten'd by Cement the orifice to the plate, and though the inverted Vessel, by reason of its stiffness and thickness and the convexity of its superficies, were strong enough to have supported a great weight without changing its figure; yet, as soon as by an exsuction or two the remaining part of the included Air was brought to such a degree of expansion, that its weaken'd Spring was able to afford but little assistance to the tenacity and firmness of the Metal, the

the weight of the pillar of the incumbent Atmosphere (which by reason of the breadth of the Vessel was considerably wide also) did presently and notably depress the upper part of the Porringer, both lessening its capacity and changing its figure; so that instead of the Convex surface, the Receiver had before, it came to a Concave one, which new figure was somewhat, though not much, increased by the further withdrawing of the included and already rarified Air. The Experiment succeeded also with an other common Porringer of the same Metal. But in such kind of Vessels, made purposely of Iron plates, it will sometimes succeed and sometimes not, according to the Diameter of the vessel and the thickness of the plate, which was sometimes strong enough and sometimes too weak to resist the pressure of the incumbent Air. And sometimes I found also, that the vessel would be thrust in, not at the top but side-ways, in case *that* side were the only part that were

were made too thin to resist the pressure of the Ambient; which Phænomenon I therefore take notice of, that you may see, that that powerful pressure may be exercised laterally as well as perpendicularly.

Perhaps this Experiment, and that I lately recited of an Hermetically sealed Bubble, by their fitness to disprove Mr. *Hobbes*'s Doctrine, may do somewhat towards the letting him see, that he might have spar'd that not over-modest and wary expression, where speaking of the Gentlemen that meet at *Gresham-College*, (of whom I pretend not to be one of the chief) he is pleased to say, *Experimenta faciant quantum volunt, nisi Principiis utantur meis nihil proficient.* But let us, if you please, pass on to what he further alledges to prove, that the space in the exhausted Receiver, which the Vacuists suppose to be *partly* empty, is full of Air. (*Video* (says A.) *si suctor trudatur usque ad fundum Cylindri Ænei, obturenturque foramina, Secuturum esse, dum suctor retrahitur,*

locum

locum in Cylindro cavo relictum fore vacuum. Nam ut in locum ejus succedat Aer, est impossibile. To which B. answers, *Credo equidem, suctorem cum Cylindri cavi superficie satis arctè cohærere ad excludendum stramen & plumam, non autem Aerem neque Aquam. Cogita enim, quod non ita accuratè congruerent, quin undiquaque interstitium relinqueretur, quantum tenuissimi capilli capax esset. Retracto ergo suctore, tantum impelleretur Aeris, quantum viribus illis conveniret quibus Aer propter suctoris Retractionem reprimitur, idque sine omni difficultate sensibili. Quanto autem interstitium illud minus esset, tantum ingrederetur Aer velocius: Vel si contactus sit, sed non per omnia puncta, etiam tunc intrabit Aer, modò suctor majore vi retrahatur. Postremò, etsi contactus ubique exactissimus sit, vi tamen satis auctâ per cochleam ferream, tum corium cedet, tum ipsum es; atque ita quoque ingredietur Aer. Credin' tu, possibile esse duas superficies ita exactè componere, ut has compositas esse supponunt illi; aut corium ita durum esse,*

ut

ut Aeri, qui Cochleæ ope incutitur, nihil omnino cedat? Corium quanquam optimum admittit aquam, ut ipse scis, si fortè feciſti unquam iter vento & pluviâ ὑόῳ καὶ ἀήτῳ. *Itaque dubitare non potes, quin retractus Suctor tantum Aeris in Cylindrum adeoque in ipsum Recipiens incutiat, quantum ſufficit ad locum ſemper relictum* perfectè *implendum. Effectus ergo, qui oritur à Retractione ſuctoris, alius non eſt quàm ventus, ventus (inquam) vehementiſſimus, qui ingreditur undiquaque inter Suctoris ſuperficiem convexam, & Cylindri ænei concavam, proceditque (versâ claviculâ) in cavitatem globi Vitrei, ſive (ut vocatur) Recipientis.*

The Subſtance of this Ratiocination having been already propos'd by Mr. *Hobbes* in his Dialogue of the Air, the 11th page, I long ſince anſwer'd it in the 30th and ſome of the following pages of my *Examen*; and therefore I ſhall only now take notice *in tranſitu* of ſome ſlight whether additions or variations, that occur in what you have been reading. And,

firſt,

first, I see no probability in what he *gratis* asserts, that so thick a Cylinder of Brass, as made the chief part of the pump of our Engine, should yield to the Sucker, that was mov'd up and down in it, though by the help of an Iron rack; and whereas he adds, that the leather, that surrounds the more solid part of the Sucker, would yield to such a force; it seems, that that compression of the leather should by thrusting the solid parts into the pores make the leather rather less than more fit to give passage to the Air; nor would it however follow, notwithstanding Mr. *Hobbes*'s Example, that, because a Body admits Water, it must be pervious to Air: For I have several times, by ways elsewhere taught, made Water penetrate the pores of Bladders, and yet Bladders resist the passage of the Air so well, that even when Air included in them was sufficiently rarified by Heat, or by our Engine, it was necessary for the Air to break them before it could get out; which would

would not have been, if it could have escap'd through their pores. What Mr. *Hobbes* inculcates here again concerning his *ventus vehementissimus*, you will find answer'd in the place of my *Examen* I lately directed you to.

A. We may then proceed to Mr. *Hobbes*'s next Explication, which he proposes in these terms:

A. *Causam video nunc unius ex Machinæ mirabilibus, nimirum cur Suctor postquam est aliquatenus retractus & deinde amissus, subitò recurrit ad Cylindri summitatem. Nam Aer, qui vi magn. fuit impulsus, rursus per repercussionem ad externa vi eadem revertitur.*

B. *Atque hoc quidem Argument satis est etiam solum, quòd locus à suctore relictus non est Vacuus. Quid enim au attrahere aut impellere suctorem potui ad locum illum unde retractus erat, s Cylindrus fuisset vacuus? Nam ut Aeri pondus aliquod id efficere potuisset, fal sum esse satis supra demonstravi ab e quod Aer in Aere gravitare non potest. Nosti etiam, quod cum è recipiente*

Aerem

Aerem omnem (ut illi loquuntur) exegerint, possunt tamen trans vitrum id quod intus sit videre, & sonum, si quis fiat, inde audire. Id quod solum, etsi nullum aliud Argumentum esset (sunt autem multa,) ad probandum, nullum *esse in Recipiente Vacuum, abundè sufficit.*

B. Here are several things joyn'd together, which the Author had before separately alledg'd in his often-mention'd Dialogue. The *first* is, the Cause he assigns of the ascension of the Sucker forcibly depress't to the bottom of the exhausted Cylinder, and then let alone by him that pumpt; to which might be added, that this ascension succeeded, when the Sucket was clogg'd with an hundred pound weight. This Explication of Mr. *Hobbes*, you will find examin'd in the 33th and 39th, and some ensuing pages of my Discourse. And as to his denying, that the weight or pressure of the Air could drive up the Sucker in that Phænomenon, because the Air does not weigh in Air, we may see the contrary largely proved in divers

places of my *Examen*, and more particularly and expresly in the four first pages of the *third* Chapter. And whereas he says in the last place, that the visibility of Bodies included in our Receivers, and the propagation of Sound, (which, by the way, is not to be understood of all Sound that may be heard, though made in the exhausted Receiver,) are alone sufficient Arguments to prove no *Vacuum*: I have consider'd that passage in the answer I made to the like allegation in the 45th page of the *Examen*; and shall only observe here, that, since the *Vacuists* can prove, that much of the Air is pumpt out of the exhausted Receiver, and will pretend, that, notwithstanding many interspers'd Vacuities, there may be in the Receiver corporeal substance enough to transmit Light and stronger Sounds, Mr. *Hobbes* has not perform'd what he pretended, if he have but barely proved, that there may be Substances capable of conveying Light and Sound in the cavity of our Re-

Receiver, since he triumphantly asserts, *Nullum esse in Recipienti Vacuum.* But we may leave Mr. *Hobbes* and his Adversaries to dispute out this point, and go on to the next passage.

A. Which follows in these words: *Ad illud autem, quod si Vesica aliquatenus inflata in Recipiente includatur, paulo post per exuctionem aeris inflatur vehementiùs & dirumpitur, quid respondes?*

B. *Motus partium Aeris undiquaque concurrentium velocissimus & per concursum in spatiis brevissimis numeroque infinitis gyrationis velocissimæ vesicam in locis innumerabilibus simul & vi magna, instar totidem terebrarum, penetrat, præsertim si vesica, antequam immittatur, quò magis resistat aliquatenus inflata sit. Postquam autem Aer penetrans semel ingressus est, facile cogitare potes, quo pacto deinceps vesicam tendet, & tandem rumpet. Verùm si antequam rumpatur, versâ claviculâ, Aer externus admittatur, videbis vesicam propter vehementiam motus temperatam diminutâ tensione rugosiorem. Nam id quoque observatum est. Jam si hæc, quam dixi,*

causa minùs tibi videatur verisimilis, vide an tu aut alius quicunque imaginari potest, quo pacto vesica distendi & rumpi possit à viribus Vacui, id est, Nihili.

B. This Explication Mr. *Hobbes* gave us in the 19th page of his Dialogue *De Natura Aeris*, and you may find it at large confuted in the latter part of the third Chapter of my *Examen*. Nor does, what he here says in the close about the *Vires Vacui* or *Nihili*, deserve to detain us, since there is no reason at all, that the *Vacuists* should ascribe to *nothing* a power of breaking a Bladder, of whose rupture the Spring of the included Air supplies them so easily with a sufficient Cause.

After what Mr. *Hobbes* has said of the breaking of a Bladder, he proceeds to an Experiment which he judges of affinity with it, and his Academian having propos'd this Question:

Unde fit ut animalia tam cito, nimirum spatio quatuor minutorum horæ, in recipiente interficiantur?

For

For answer to it our Author says:

B. *Nonne animalia sic inclusa insugunt in Pulmones Aerem vehementissimè motum? Quo motu necesse est ut transitus sanguinis ab uno ad alterum cordis ventriculum interceptus, non multò pòst sistatur. Cessatio autem sanguinis, Mors est. Possunt tamen animalia cessante sanguine revivíscere, si Aer externus satis maturè intromittatur, vel ipsa in Aerem temperatum, antequam refrixerit sanguis, extrahantur.*

This Explication is not probable enough, to oblige me to add any thing about it to what I have said in the 49th and the two following pages of my *Examen*; especially the most vehement motion, ascrib'd to the Air in the Receiver, having been before proved to be an Imaginary thing. You may therefore, if you please, take notice of the next Explication.

[*Idem Aer* (says he) *in Recipiente Carbones ardentes extinguit, sed & illi, si, dum satis calidi sunt, eximantur, relucebunt. Notissimum est, quòd in fodinis Carbonum terreorum (cujus rei ex-*

perimentum ipse vidi) sæpissime è lateribus foveæ ventus quidam undiquaque exit, qui fossores interficit ignemque extinguit, qui tamen reviviscunt si satis cito ad Aerem liberum extrahantur.]

This Comparison which Mr. *Hobbes* here summarily makes, he more fully display'd in his Dialogue *De Natura Aeris*, and I consider'd, what he there alledg'd, in the 52th page and the two next of my *Examen*. And, though I will not contradict Mr. *Hobbes* in what he historically asserts in this passage; yet I cannot but somewhat doubt, whether he mingles not his conjecture with the bare matter of fact. For, though I have with some curiosity visited Mines in more places than one, and propos'd Questions to Men that have been conversant in other Mines, both elsewhere and in *England* (and particularly in *Derbyshire* where Mr. *Hobbes* lived long;) yet I could never find, that any such odd and vehement wind, as Mr. *Hobbes* ascribes the Phænomenon to, had been

been by them observed to kill the Diggers, and extinguish well-lighted Coals themselves: And indeed, it seems more likely, that the damp, by its tenacity or some peculiarly malign quality, did the mischief, than a wind, of which I found not any notice taken; especially since we see, what vehement winds Men will be able to endure for a long time, without being near-kill'd by them; and that it seems very odd, that a wind, that Mr. *Hobbes* does not observe to have blown away the Coals, that were let down, should be able (instead of kindling them more fiercely) to blow them *out*.

A. The last Experiment of your Engine, that your Adversary mentions in these Problems, is deliver'd in this passage:

A. *Si phialam aquæ in Recipiens dimiseris, exucto Aere bullire videbis aquam. Quid ad hoc Respondebis?*

B. *Credo sanè in tanta Aeris motitatione saltaturam esse aquam, sed ut calefiat nondum audivi. Sed imagina-*

bile non est, Saltationem illam à Vacuo nasci posse.

B. This Phænomenon he likewise took notice of, and attempted to explicate in his above-mention'd Dialogue, which gave me occasion in the 46th and 47th pages of my *Examen*, to shew how unlikely 'tis, that the vehement motion of the Air should be the cause of it; but he here tells us, that 'tis not imaginable, that this dancing of the water (as he is pleas'd to call it) proceeds from a *vacuum*, nor do I know any Man that ever pretended, that a *vacuum* was the efficient cause of it. But the *Vacuists* perhaps will tell him, that, though the bubbling of the water be not an *effect* of a *vacuum*, it may be a *proof* of it against him; for they will tell him, that it has been formerly proved, that a great part of the Atmospherical Air is by pumping remov'd out of our exhausted Receiver, and consequently can no more, as formerly, press upon the surface of the water. Nor does Mr. *Hobbes*

shew

shew what succeeds in the room of it; and therefore it will be allowable, for them to conclude against him (though not perhaps against the *Cartesians*) that there are a great many interspers'd Vacuities left in the Receiver, which are the occasion, though not the proper efficient cause, of the Phænomenon. For they will say, that the Springy Particles of the yet included Air, having room to unbend themselves in the spaces deserted by the Air that was pumpt out, the Aerial and Springy Corpuscles, that lay conceal'd in the pores of the water, being now freed from the wonted pressure that kept them coil'd up in the liquor, expanded themselves into numerous bubbles, which, because of their comparative lightness, are extruded by the water, and many of them appear to have risen from the bottom of it. And Mr. *Hobbes*'s vehement wind, to produce the several Circumstances of this Experiment, must be a lasting one. For, after the agitation of the Pump has been

been quite left off, provided the external Air be kept from getting in, the bubbles will sometimes continue to rise for an hour after. And that which agrees very well with our Explication and very ill with that of Mr *Hobbes*'s, is, that, when by having continued to pump a competent time, the water has been freed from the Aerial particles that lurk'd in it before, though one continue to pump as lustily as he did, yet the water will not at all be cover'd with bubbles as it was, the Air that produc'd them being spent; though, according to Mr. *Hobbes*'s Explication, the wind in the Receiver continuing, the dance of the water should continue too.

A. I easily ghess, by what you have said already, what you may say of that *Epiphonema* wherewith Mr. *Hobbes* (in his 18th page) concludes the Explications of the Phænomena of your Engine. [*Spero jam te certum esse*, says he, *nullum esse Machinæ illius Phænomenon, quo demonstrari potest ullum in Universo locum dari corpore omni vacuum.*]

B. If

B. If you ghefs'd aright, you ghefs'd that I would fay, that as to the Phænomena of my Engine, my buſineſs was to prove, that he had not ſubſtituted good Explications of them in the place of mine, which he was pleaſed to reject. And as for the proving a *Vacuum* by the Phænomena of my Engine, though I declar'd that was not the thing intended, yet I ſhall not wonder, that the *Vacuiſts* ſhould think thoſe Phænomena give them an advantage againſt Mr. *Hobbes.* For, though in the paſſage recited by you he ſpeak more cautiouſly than he is wont to do, yet, by what you may have already obſerv'd in his Argumentations, the way he takes to ſolve the Phænomena of our Engine, is by contending, that our Receiver, when we ſay it is almoſt exhauſted, is as full as ever (for he will have it *perfectly* full,) of common Air; which is a conceit ſo contrary to I know not how many Phænomena, that I do not remember I have met with or

heard

heard of any Naturalist, whether Vacuist or Plenist, that having read my Physico-Mechanical Experiments and his Dialogue, has embrac'd his opinion.

A. After what you have said, I will not trouble you with what he subjoyns about *Vacuum* in general, where having made his Academian say, [*Mundum scis finitum esse, & per Consequens vacuum esse oportere totum illud Spatium quod est extra mundum infinitum. Quid impedit quo minus vacuum illud cum Aere mundano permiscea-tur?*] He answers: *De rebus transmundanis nihil scio.* For I know, that it concerns not you to take notice of it. But possibly the *Vacuists* will think, he fathers upon them an Impropriety they would not be guilty of, making them speak, as if they thought, the *ultra-mundan Vacuum* were a real Substance that might be brought into this World and mingled with our Air. And since, for ought I know, Mr. *Hobbes* might have spar'd this passage, if he had not de-

sign'd

ſign'd it ſhould introduce the ſlighting anſwer he makes to it; I ſhall add, that by the account Mr. *Hobbes* has given of ſeveral Phænomena *within* the World, 'tis poſſible, that the Vacuiſts may believe his Profeſſion of knowing nothing of things *beyond* it.

After the *Experimenta Boyliana* (as your other Adverſary calls them;) Mr. *Hobbes* proceeds to the Torricellian Experiment, of which he thus diſcourſes:

A. *Quid de experimento cenſes Torricelliano, probante Vacuum per Argentum vivum hoc modo: eſt in ſeq. figura ad* A, *pelvis ſive aliud vas, & in eo Argentum vivum uſque ad* B; *eſt autem* C D *tubus vitreus concavus repletus quoque Argento vivo. Hunc tubum ſi digito obturaveris erexeriſque in vaſe* A, *manumque abſtuleris, deſcendet Argentum vivum à* C; *verùm non effundetur totum in pelvim, ſed ſiſtetur in diſtantia quadam, puta in* D. *Nonne ergo neceſſarium eſt, ut pars tubi inter* C & D *ſit vacua? Non enim puto negabis*

quin

quin superficies tubi concava & Argenti vivi convexa se mutuo exquisitissimè contingant.

B. *Ego neque nego contactum, neque vim Consequentiæ intelligo.*

By which passage it seems that he still persists in the solution of this Experiment, which he gave in his Dialogue *De Naturâ Aeris*, and formerly did, for the main, either propose, or adopt, in his Elements of Philosophy.

B. This opinion or explication of Mr. *Hobbes* I have, as far as concerns me, consider'd in the 36th, and some insuing pages, of my *Examen*, to which it may well suffice me to refer you. But yet let me take notice of what he now alledges:

B. *Si quis* (says he) *in Argentum vivum, quod in vase est, vesicam immerserit inflatam, nonne illa amotâ manu emerget?*

A. *Ita certè, etsi esset vesica ferrea vel ex materia quacunque præter Aurum.*

B. *Vides igitur ab Aere penetrari posse Argentum vivum.*

A. *Etiam,*

A. *Etiam, & quidem illâ ipsâ vi quam à pondere accipit Argenti vivi.*

I confeſs this Allegation did a little ſurprize me: It concern'd Mr. *Hobbes* to prove, that as much Air, as was diſplac'd by the deſcending Mercury, did at the orifice of the Tube, immers'd in ſtagnant Mercury, inviſibly aſcend to the upper part of the pipe. To prove this he tells us, that a bladder full of Air being depreſs'd in Quickſilver, will, when the hand that depreſs'd it is remov'd, be ſqueez'd up by the very weight of the Mercury, whence it follows, that Air may penetrate Quickſilver. But I know not, who ever deny'd, that Air inviron'd with Quickſilver may thereby be ſqueez'd upwards; but, ſince even very ſmall bubbles of Air may be ſeen to move in their paſſage through Mercury, I ſee not, how this Example will at all help the Propoſer of it. For 'tis by meer accident, that the Air included in the bladder comes to be buoy'd up, becauſe the bladder it ſelf is ſo; and if

it

it were fill'd with Water instead of Air, or with Stone instead of Water, it would nevertheless emerge, as himself confesses it would do, if it were made of Iron, or of any Matter besides Gold, because all other Bodies are lighter *in specie* than Quicksilver. But since the emersion of the bladder is manifest enough to the sight, I see not how it will serve Mr. *Hobbes*'s turn, who is to prove that the Air gets into the Torricellian Tube invisibly; since 'tis plain, that even heedful observation can make our Eyes discover no such trajection of the Air; which (to add that inforcement of our Argument) must not only pass unseen through the sustained Quicksilver, but must likewise unperceivedly dive, in spite of its comparative lightness, beneath the surface of the ponderous stagnant Mercury, to get in at the orifice of the erected Tube. But let us, if you please, hear the rest of his Discourse about this Experiment.

A. Though it be somewhat prolix,

lix, yet, according to my cuſtom hitherto, I will give it you *verbatim.*

B. *Simul atque Argentum vivum deſcenderit ad* D, *altius erit in vaſe* A *quàm antè, nimirum plus erit Argenti vivi in vaſe quàm erat ante deſcenſum, tanto quantum capit pars tubi* C, D. *Tanto quoque minus erit Aeris extra tubum quàm ante erat. Ille autem Aer qui ab Argento vivo loco ſuo extruſus eſt, (ſuppoſitâ univerſi plenitudine) quò abire poteſt niſi ad eum locum, qui in tubo inter* C *&* D *à deſcenſu Argenti vivi relinquebatur? ſed quâ, inquies, viâ in illum locum ſucceſſurus eſt? Quà, niſi per ipſum corpus Argenti vivi Aerem urgentis? Sicut enim omne grave liquidum, ſuî ipſius ponderè, Aerem, quem deſcendendo premit, aſcendere cogit (ſi via alia non detur) per ſuum ipſius corpus; ita quoque Aerem quem premit aſcendendo, (ſi via alia non detur) per ſuum ipſius corpus tranſire cogit. Manifeſtum igitur eſt, ſuppoſitâ mundi plenitudine poſſe Aerem externum ab ipſa gravitate Argenti vivi cogi in locum illum inter* C *&* D. *Itaque Phænomenon illud neceſſitatem vacui non*

demonstrat. Quoniam autem corpus Argenti vivi penetrationi, quæ fit ab Aere, non nihil resistit, & ascensioni Argenti vivi in vase A *resistit Aer; quando illæ duæ resistentiæ æquales erunt, tunc in tubo sistetur alicubi Argentum vivum; atque ibi est* D.

B. In answer to this Explication I have in my *Examen* propos'd divers things, which you may there meet with: And indeed his Explication has appear'd so improbable to those that have written of this Experiment, that I have not found it embrac'd by any of them, though, when divers of them oppos'd it, the Phænomena of our Engine were not yet divulg'd. Not then needlesly to repeat what has been said already, I shall on this occasion only add one Experiment, that I afterwards made, and it was this: Having made the Torricellian Experiment (in a straight Tube) after the ordinary way, we took a little piece of a fine Bladder, and raising the Pipe a little in the stagnant Mercury, but not so high

as

as the ſurface of it, the piece of Bladder was dexterouſly conveyed in the Quickſilver, ſo as to be applied by ones finger to the immerſed orifice of the Pipe, without letting the Air get into the Cavity of it; then the Bladder was tyed very ſtraight and carefully to the lower end of the Pipe, whoſe orifice (as we ſaid) it cover'd before, and then the Pipe being ſlowly lifted out of the ſtagnant Mercury, the impendent Quickſilver appear'd to lean but very lightly upon the Bladder, being ſo near an exact Æquilibrium with the Atmoſperical Air, that, if the Tube were but a very little inclin'd, whereby the gravitation of the Quickſilver, being not ſo perpendicular, came to be ſomewhat leſſen'd, the Bladder would immediately be driven into the orifice of the Tube, and to the Eye, plac'd without, appear to have acquir'd a concave ſuperficies inſtead of the convex it had before. And when the Tube was re-erected, the Bladder would no longer appear

ſuck'd in, but be again ſomewhat protuberant. And if, when the Mercury in the Pipe was made to deſcend a little below its ſtation into the ſtagnant Mercury, if, I ſay, at that nick of time the piece of Bladder were nimbly and dexterouſly apply'd, as before, to the immers'd orifice, and faſten'd to the ſides of the Pipe, upon the lifting the Inſtrument out of the ſtagnant Mercury, the Cylinder of that Liquor being now ſomewhat ſhort of its due height, was no longer able fully to counterpoiſe the weight of the Atmoſpherical Air, which conſequently, though the Glaſs were held in an erected poſture, would preſs up the Bladder into the orifice of the Pipe, and both make and maintain there a Cavity ſenſible both to the Touch and the Eye.

A. What did you mainly drive at in this Experiment?

B. To ſatisfie ſome Ingenious Men, that were more diffident of, than skilful in, Hydroſtaticks, that the

the preſſure of the external Air is capable of ſuſtaining a Cylinder of 29 or 30 Inches of Mercury, and upon a ſmall leſſening of the gravitation of that ponderous liquor, to preſs it up higher into the Tube. But a farther uſe may be made of it againſt Mr. *Hobbes*'s pretenſion. For, when the Tube is again erected, the Mercury will ſubſide as low as at firſt, and leave as great a ſpace as formerly was left deſerted at the top; into which how the Air ſhould get to fill it, will not appear eaſie to them, that, like you and me, know by many tryals, that a Bladder will rather be burſt by Air than grant it paſſage. And if it ſhould be pretended, *either* that ſome Air from without had yet got through the Bladder, *or* that the Air, that they may preſume to have been juſt before included between the Bladder and the Mercury, made its way from the lower part of the Inſtrument to the upper; 'tis obvious to anſwer, That 'tis no way likely, that it ſhould paſs all along the Cy-

linder unseen by us; since, when there are really any Aerial Bubbles, though smaller than Pins heads, they are easily discernible. And in our case, there is no such resistance of the Air to the ascension of the stagnant Mercury, as Mr. *Hobbes* pretends in the Torricellian Experiment made the usual way.

A. But, whatever becomes of Mr. *Hobbes*'s Explication of the Phænomenon; yet may not one still say, that it affords no advantage to the *Vacuists* against him?

B. Whether or no it do against other *Plenists*, I shall not now consider; but I doubt, the *Vacuists* will tell Mr. *Hobbes*, that he is fain in two places of the Explication, we have read, to suppose the Plenitude of the World, that is, to beg the thing in question, which 'tis not to be presum'd they will allow.

A. But may not Mr. *Hobbes* say, that 'tis as lawful for him to suppose a *Plenum*, as for them to suppose a *Vacuum*.

B. I think he may justly say so; but 'tis like they will reply, that, in their way of explicating the Torricellian Experiment, they do not *suppose* a *Vacuum* as to Air, but *prove* it. For they shew a great space, that having been just before fill'd with Quicksilver, is now deserted by it, though it appeared not, that any Air succeeded in its room; but rather, that the upper end of the Tube is either totally or near totally so devoid of Air, that the Quicksilver may without resistance, by barely inclining the Tube, be made to fill it to the very top: Whereas Mr. *Hobbes* is fain to have recourse to that which he knows they deny, the Plenitude of the World, not proving by any sensible Phænomena, that there *did* get in through the Quicksilver Air enough to fill the deserted part of the Tube, but only concluding, that so much Air *must* have got in there, because, the World being full, it could find no room any where else; which the *Vacuists* will take for no

proof at all, and the *Cartesians*, though *Plenists*, who admit an Etherial matter capable of passing through the pores of Glass, will, I doubt, look upon but as an improper Explication.

A. I remember on this occasion another Experiment of yours, that seems unfavourable enough to Mr. *Hobbes*'s Explication, and you will perhaps call it to mind when I tell you, that 'twas made in a bended Pipe almost fill'd with Quicksilver.

B. To see whether we understand one another, I will briefly describe the Instrument I think you mean. We took a Cylindrical Pipe of Glass, clos'd at the upper end, and of that length, that being dexterously bent at some Inches from the bottom, the shorter legg was made as parallel as we could to the longer: In this Glass we found an expedient, (for 'tis not easie to do,) to make the Torricellian Experiment, the Quicksilver in the shorter legg serving instead of the stagnant Quicksilver in the usual Baroscope, and the Quicksilver in the longer

longer legg reaching above that in the shorter about eight or nine and twenty Inches. Then, by another artifice, the shorter legg, into which the Mercury did not rise within an Inch of the top, was so order'd, that it could in a trice be Hermetically seal'd, without disordering the Quick-silver. And this is the Instrument that I ghess you mean.

A. It is so, and I remember, that it is the same with that, which in the Paradox about Suction you call, whilst the shorter legg remains unseal'd, a *Travelling Baroscope.* But when I saw you make the Experiment, that legg was Hermetically seal'd, an Inch of Air in its natural or usual consistence being left in the upper part of it, to which Air you outwardly applied a pair of heated Tongs.

B. Yet that, which I chiefly aim'd at in the Trial, was not the Phænomenon I perceive you mean; for, my design was, by breaking the Ice for them, to encourage some, that may have

have more skill and accommodation than I then had, to make an attempt that I did not find to have been made by any; namely, to reduce the Expansive force of Heat in every way included Air, if not in some other Bodies also, to some kind of measure, and, if 'twere possible, to determin it by weight. And I presumed, that at least the event of my Tryal would much confirm several Explications of mine, by shewing, that Heat is able, as long as it lasts, very considerably to increase the Spring or pressing power of the Air. And in this conjecture I was not mistaken; for, having shut up, after the manner newly recited, a determinate quantity of uncompress Air, which, (in the Experiment you saw,) was about one Inch; we warily held a pair of heated Tongs near the outside of the Glass, (without making it touch the Instrument, for fear of breaking it,) whereby the Air being agitated was enabled to expand it self to double its former Dimensions, and consequently

quently had its Spring so strengthen'd by Heat, that it was able to raise all the Quicksilver in the longer legg, and keep up or sustain a Mercurial Cylinder of about nine and twenty Inches high, when by its expansion it would, if it had not been for the Heat, have lost half the force of its elasticity. But whatever I design in this Experiment, pray tell me, what use you would make of it against Mr. *Hobbes.*

A. I believe, he will find it very difficult to shew, what keeps the Mercury suspended in the longer legg of the *Travelling Baroscope*, when the shorter legg is unstopt, at which it may run out; since this Instrument may, as I have try'd, be carried to distant places, where it cannot with probability be pretended, that any Air has been displac'd by the fall of the Quicksilver in the longer legg, which perhaps fell long before above a mile off. And when the shorter legg is seal'd, it will be very hard for Mr. *Hobbes* to shew there the odd motions

motions of the Air, to which he ascribes the Torricellian Experiment. For, if you warily incline the Instrument, the Quicksilver will rise to the top of the longer legg, and immediately subside, when the Instrument is again erected, and yet no Air appears to pass through the Quicksilver interpos'd between the ends of the longer and the shorter legg. But that which I would chiefly take notice of in the Experiment, is, that upon the external application of a hot Body to the shorter legg of the Baroscope, when 'twas seal'd up, the included Air was expanded from one Inch to two, and so rais'd the whole Cylinder of Mercury in the longer legg, and, whilst the heat continued undiminished, kept it from subsiding again. For, if the Air were able to get unseen through the body of the Quicksilver, why had it not been much more able, when rarified by Heat, to pass through the Quicksilver, than for want of doing so to raise and sustain so weighty a Cylinder

der of Mercury? I shall not stay to inquire on this occasion, how Mr. *Hobbes* will, according to his *Hypothesis*, explicate the rarefaction of the Air to double its former dimensions, and the condensation of it again; especially since, asserting that part of the upper legg, that is unfill'd with the Quicksilver, to be perfectly full of Air, he affirms that, which I doubt he cannot prove, and which may very probably be disproved by the Experiment you mention in the Discourse about *Suction*, where you shew, to another purpose, that in a *Travelling Baroscope*, whose shorter legg is seal'd, if the end of the longer legg be open'd, whereby it comes *indeed* to be fill'd with Air, the pressure of that Air will enable the subjacent Mercury notably to compress the Air included in the shorter legg.

B. I leave Mr. *Hobbes* to consider what you have objected against his Explication of the Torricellian Experiment; to which I shall add nothing,

thing, though perhaps I could add much, becauſe I think it may be well ſpared, and our Conference has laſted long already.

A. I will then proceed to the laſt Experiment recited by Mr. *Hobbes* in his *Problemata de Vacuo.*

A. *Si Phialam, collum habentem longiuſculum, eandémque omni Corpore præter Aerem vacuam ore ſugas, continuoque Phialæ os aquæ immergas, videbis aquam aliquouſque aſcendere in Phialam. Quî fieri hoc poteſt niſi factum ſit Vacuum ab exuctione Aeris, in cujus locum poſſit Aqua illa aſcendere?*

B. *Conceſſo Vacuo, oportet quædam loca vacua fuiſſe in illo Aere, etiam qui erat intra Phialam ante ſuctionem. Cur ergo non aſcendebat Aqua ad ea implenda abſque ſuctione? Is qui ſugit Phialam, neque in ventrem quicquam, neque in pulmones, neque in os è Phiala exugit. Quid ergo agit? Aerem commovet, & in partibus ejus conatum ſugendo efficit per os exeundi, & non admittendo, conatum redeundi. Ab his conatibus contrariis componitur circumitio intra*

Phialam,

Phialam, & conatus exeundi quaquaversum. Itaque Phialæ ore aquæ immerso, Aer in subjectam aquam penetrat è Phiala egrediens, & tantundem aquæ in Phialam cogit.

Præterea vis illa magna suctionis facit, ut sugentis labra cum collo Phialæ aliquando arctissimè cohæreant propter contactum exquisitissimum.

B. As to the first Clause of Mr. *Hobbes*'s account of our Phænomenon, the *Vacuists* will easily answer his Question by acknowledging, that there were indeed interspers'd Vacuities in the Air contain'd in the Vial before the suction; but they will add, there was no reason, why the Water should ascend to fill them, because, being a heavy body, it cannot rise of it self, but must be raised by some prevalent weight or pressure, which then was wanting. Besides, that there being interspers'd Vacuities as well in the rest of the Air that was very near the Water, as in that contained in the Vial, there was no reason, why the Water

should

should ascend to fill the Vacuities of one portion of Air rather than those of another. But when once by suction a great many of the Aerial Corpuscles were made to pass out of the Vial, the Spring of the remaining Air being weaken'd, whilst the pressure of the ambient Air, which depends upon its constant Gravity, is undiminished, the Spring of the internal becomes unable to resist the weight of the external Air, which is therefore able to impel the interpos'd Water with some violence into the Cavity of the Glass, 'till the Air, remaining in that Cavity, being reduced almost to its usual Density, is able by its Spring, and the weight of the Water got up into the Vial, to hinder any more Water from being impell'd up. For, as to what Mr. *Hobbes* affirms, that, *Is qui sugit Phialam neque in ventrem quicquam, neque in pulmones, neque in os quicquam exugit*: How it will agree with what he elsewhere delivers about Suction, I leave him to consider.

But

But I confeſs, I cannot but wonder at his confidence, that can poſitively affert a thing ſo repugnant to the common ſentiments of Men of all opinions, without offering any proof for it. But I ſuppoſe, they that are by tryal acquainted with Sucking, and have felt the Air come in at their mouths, will prefer their own experience to his authority. And as to what he adds, that the Perſon that ſucks agitates the Air, and turns it within the Vial into a kind of circulating wind, that endeavours every where to get out; I wiſh, he had ſhewn us by what means a Man that ſucks makes this odd Commotion of the Air, eſpecially in ſuch Vials as I uſe to employ about the Experiment, the orifice of whoſe neck is ſometimes leſs than a Pins head.

A. That there may be really Air extracted by Suction out of a Glaſs, me thinks you might argue from an Experiment I ſaw you make with a Receiver which was exhauſted by your Pump, and conſequently by Su-

ction. For I remember, when you had counterpois'd it with very good Scales, and afterwards by turning a stop-cock, let in the outward Air, there rush'd in as much Air to fill the space that had been deserted by the Air pumpt out, as weighed some scruples (consisting of twenty grains a piece) though the Receiver were not of the largest size.

B. You did well to add that Clause; for, the *Magdeburgic* Experiment, mentioned by the industrious *Schottus*, having been made with a vast Receiver, the readmitted Air amounted to a whole ounce and some drachms. But to return to Mr. *Hobbes*, I fear not that he will perswade you, that have seen the Experiment he recites, that as soon as the neck of the Vial is unstopt under water, the Air, that whirl'd about before, makes a sally out, and forces in as much water. For, if the orifice be any thing large, you will, instead of feeling an endeavour to thrust away your finger that stopt it, find the pulp of

your

your finger so thrust inward, that a *Peripatetick* would affirm that he felt it suckt in. And that Intrusion may be the Reason, why the lip of him that sucks is oftentimes strongly fasten'd to the orifice of the Vials neck, which Mr. *Hobbes* ascribes to a most exquisit contact, but without clearly telling us, how that extraordinary contact is effected. And when your finger is removed, instead of perceiving any Air go out of the Vial through the water, (which, if any such thing happen, you will easily discover by the bubbles,) you shall see the water briskly spring up in a slender stream to the top of the Vial, which it could not do, if the Cavity were already full of Air. And to let you see, that, when the Air does really pass in or out of the Vial immers'd under water, 'tis very easie to perceive its motions, if you dip the neck of the Vial in water, and then apply to the globulous part of it either your warm hands or any other competent Heat, the internal

Air being rarified; you shall see a portion of it, answerable to the degree of Heat you applied, manifestly pass through the water in successive bubbles, whilst yet you shall not see any water get into the Vial to supply the place deserted by that Air. And if, when you have (as you may do by the help of sucking) fill'd the neck and part of the belly of the Vial with water, you immerse the orifice into stagnant water, and apply warm hands to the globulous part as before, you will find the water in the Vial to be driven out, before any bubbles pass out of the Vial into the surrounding water; which shews, that the Air is not so forward to dive under the water, (and much less under so ponderous a liquor as Quicksilver,) as Mr. *Hobbes* has supposed.

A. That 'tis the Pressure of the external Air, that (surmounting the Spring of the internal) drives up the water into the Vial we have been speaking of, does, I confess, follow upon your Hypothesis: But an Experimen-

rimentarian Philoſopher, as Mr. *Hobbes* calls you among others, may poſſibly be furniſhed with an Experiment to confirm this to the Eye.

B. You bring into my mind what I once deviſed to confirm my Hypotheſis about Suction, but found a while ſince that I had omitted it in my Diſcourſe about that Subject. And therefore I ſhall now repeat to you the ſubſtance at leaſt of the Memorial that was written of that Experiment, by which the great intereſt of the weight of the Atmoſpherical Air in Suction will appear, and in which alſo ſome things will occur, that will not well agree with Mr. *Hobbes*'s Explication, and prevent ſome of his Allegations againſt mine.

A. Having not yet met with an Experiment of this nature, ſuch an one as you ſpeak of will be welcome to me.

B. We took a Glaſs Bubble, whoſe long ſtem was both very ſlender and very Cylindrical; then by applying

to the outſide of the Ball or globu-
lous part a convenient heat, we ex-
pell'd ſo much of the Air, as that,
when the end of the pipe was dipt
in water, and the inward Air had
time to recover its former coolneſs,
the water aſcended either to the top
of the pipe or very near it. This
done, we gently and warily rarified
the Air in the Cavity of the Bubble,
'till by its expanſion it had driven out
almoſt all the water that had got up
into the ſtem, that ſo it might attain
as near as could be to that degree of
heat and meaſure of expanſion, that
it had when the water began to riſe
in it. And we were careful to leave
two or three drops of water unex-
pell'd at the bottom of the pipe, that
we might be ſure, that none of the
included Air was by this ſecond ra-
refaction driven out at the orifice of
it; as the depreſſion of the water ſo
low aſſured us, on the other ſide,
that the included Air wanted nothing
conſiderable of the expanſion it had
when the water began to aſcend into
the

the pipe. Whilst the Air was in this rarified state, we presently removed the little Instrument out of the stagnant water into stagnant Quicksilver, which in a short time began to rise in the pipe. Now, if the ascension of the liquor were the effect of Natures Abhorrence of a *Vacuum*; or of some internal principle of Motion; or of the Compression and propagated Pulsion of the outward Air by that which had been expell'd; why should not the Mercury have ascended to the top of the pipe, as the water did before? But *de facto* it did not ascend half, or perhaps a quarter so far; and if the pipe had been long enough, as well as 'twas slender enough, I question, whether the Mercury would have ascended (in proportion to the length of the stem) half so high as it did.

Now of this Experiment, which we tryed more than once, I see not, for the reason lately express'd, how any good account will be given without our *Hypothesis*, but according to *That* 'tis clear.

A. I think I perceive why you say so; for the Ascension of Liquors being an effect of the prevalency of the external Airs pressure against the resistance it meets with in the Cavity of the Instrument, and the Quicksilver being bulk for bulk many times heavier than water, the same surplusage of pressure that was able to impel up water to the top of the pipe, ought not to be able to impel up the Quicksilver to any thing near that height. And if it be here objected, as it very plausibly may be, that the raised Cylinder of Mercury was much longer than it ought to have been in reference to a Cylinder of Water, the proportion in gravity between those two Liquors (which is almost that of fourteen to one) being considered; I answer, that when the Cylinder of Water reach'd to the pipe, the Air possess'd no more than the Cavity of the globulous part of the Instrument, being very little assisted to dilate it self by so light a Cylinder as that of Water: But when the

the Quickſilver came to be impell'd into the Inſtrument by the weight of the external Air, that ponderous Body did not ſtop its aſcent as ſoon as it came to be equiponderant to the formerly expell'd Cylinder of Water; becauſe, to attain that height, it reached but a little way into the pipe, and left all the reſt of the Cavity of the pipe to be fill'd with part of that Air, which formerly was all ſhut up in the Cavity of the Bubble; by which means the Air, included in the whole Inſtrument, muſt needs be in a ſtate of expanſion, and thereby have its Spring weakened, and conſequently diſabled to reſiſt the preſſure of the external Air, as much as the ſame included Air did before, when it was leſs rarified; on which account, the undiminiſhed weight or preſſure of the external Air was able to raiſe the Quickſilver higher and higher, 'till it had obtained that height, at which the preſſure, compounded of the weight of the Mercurial Cylinder and the Spring of the internal

internal Air (now less rarified than before,) was equivalent to the pressure of the Atmosphere or external Air.

B. You have given the very Explication I was about to propose; wherefore I shall only add, that, to confirm this Experiment by a kind of Inversion of it, we drove by heat a little Air out of the Bubble, and dipt the open end of the pipe into Quicksilver, which by this means we made to ascend 'till it had fill'd about a fourth part or less of the pipe, when that was held erected. Then carefully removing it without letting fall any Quicksilver, or letting in any Air, we held the orifice of the pipe a little under the surface of a Glass full of Water, and applying a moderate heat to the outside of the Ball, we warily expell'd the Quicksilver, yet leaving a little of it to make it sure that no Air was driven out with it; then suffering the included Air to cool, the external Air was found able to make the

Water

Water not only ascend to the very top of the pipe, and thence spread it self a little into the Cavity of the Ball, but to carry up before it the Quicksilver that had remained unexpell'd at the bottom of the stem. And if in making the Experiment we had first raised, as we sometimes did, a greater quantity of Quicksilver, and afterwards drove it out, the quantity of Water, that would be impell'd into the Cavity of the pipe and ball, would be accordingly increased.

A. In this Experiment 'tis manifest, that something is driven out of the Cavity of the Glass before the Water or Quicksilver begins to ascend in it: And here also we see not, that the Air can pass through the pores of Quicksilver or Water, but that it drives them on before it, without sensibly mixing with them. In this Experiment there appears not at all any Circular Wind, as Mr. *Hobbes* fancies in the suckt Vial we are disputing of, nor any tendency outwards of

of the included Air upon the account of such a Wind; but, instead of these things, that the ascension of the Liquors into the Cavity of the pipe depends upon the external Air, pressing up the Liquors into that Cavity, may be argu'd by this, that the same weight of the Atmosphere impell'd up into the pipe so much more of the lighter Liquor, *Water*, than of the heavier Liquor, *Mercury*.

B. You have said enough on this Experiment; but 'tis not the only I have to oppose to Mr. *Hobbes* his Explication: For, that there is no need of the sallying of Air out of a Vial, to make the Atmospherical Air press against a Body that closes the orifice of it, when the pressure of the internal Air is much weakened; I have had occasion to shew some *Virtuosi*, by sucking out, with the help of an Instrument, a considerable portion of the Air contained in a Glass; for having then, instead of unstopping the orifice under water, nimbly applied a flat Body to it, the external Air press'd

that

that Body so forcibly against it, as to keep it fastened and suspended, though 'twere clogg'd with a weight of many ounces.

A. Another Experiment of yours Mr. *Hobbes*'s Explication brings into my mind, by which it appears, that, if there be such a Circular Wind, as he pretends, produced by Suction in the Cavity of the Vial, it must needs be strangely lasting. For I have seen more than once, that, when you have by an Instrument suckt much of the Air out of a Vial, and afterwards carefully closed it, though you kept the slender neck of it stopt a long time, perhaps for some weeks or months, yet when 'twas open'd under water, a considerable quantity of the Liquor would be briskly impell'd up into the neck and belly of the Vial. So that, though I will not be so pleasant with Mr. *Hobbes*, as to mind you on this occasion of those Writers of Natural Magick, that teach us to shut up Articulate Sounds in a Vessel, which being transported to a distant

place

place and open'd there, will render the Words that are committed to it; yet I must needs say, that so lasting a Circular Wind, as, according to Mr. *Hobbes*, your Experiments exhibited, may well deserve our wonder.

B. Your admiration would perchance increase, if I should assure you, that having with the Sun-beams produced smoak in one of those well-stopt Vials, this Circular Wind did not at all appear to blow it about, but suffered it to rise, as it would have done if the included Air had been very calm. And now I shall add but one Experiment more, which will not be liable to some of the things as invalid as they are, which Mr. *Hobbes* has alledged in his account of the Vial, and which will let you see, that the weight of the Atmospherical Air is a very considerable thing; and which may also incline you to think, that, whilst Mr. *Hobbes* does not admit a subtiler Matter than common Air to pass through the Pores of close and solid Bodies, the Air he has recourse to will some-

ſometimes come too late to prevent a *Vacuum.* The Experiment, which was partly accidental, I lately found regiſtred to this ſenſe, if not in theſe words: [Having, to make ſome Diſcovery of the weight of the Air, and for other purpoſes, caus'd an Æolipile, very light conſidering its bulk, to be made by a famous Artiſt, I had occaſion to put it ſo often into the fire for ſeveral Tryals, that at length the Copper ſcal'd off by degrees, and left the Veſſel much thinner than when it firſt came out of the Artificers hands; and a good while after, this change in the Inſtrument being not in my thoughts, I had occaſion to imploy it, as formerly, to weigh how many grains it would contain of the Air at ſuch a determinate conſtitution of the Atmoſphere, as was to be met with, where I then chanced to be. For the making this Experiment the more exactly, the Air was by a ſtrong, but warily applied, fire ſo carefully driven away, that, when clapping a piece of Sealing-wax to the Pin-hole, at which it had been forced out, we hindred any com-

communication betwixt the Cavity of the Instrument and the external Air; we suppos'd the Æolipile to be very well exhausted, and therefore laid it by, that, when it should be grown cold, we might, by opening the orifice with a Pin, again let in the outward Air, and observe the encrease of weight that would thereupon ensue: But the Instrument, that, as I was saying, was grown thin, had been so diligently freed from Air, that the very little that remain'd, and was kept by the Wax from receiving any assistance from without, being unable by its Spring to assist the Æolipile to support the weight of the ambient Air; this external fluid did by its weight press against it so strongly, that it compres's'd it, and thrust it so considerably inwards, and in more than one place so chang'd its figure, that, when I shew'd it to the *Virtuosi* that were assembled at *Gresham-Colledge*, they were pleased to command it of me to be kept in their Repository, where I presume it is still to be seen.

FINIS.

OF THE CAUSE OF *ATTRACTION* BY Suction, A PARADOX.

By the Honourable
ROBERT BOYLE,
Fellow of the *Royal Society*.

LONDON,
Printed by *William Godbid*, and are to be Sold by *Moses Pitt*, at the *Angel* over against the little North Door of St. *Paul's* Church. 1674.

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OF THE CAUSE OF Attraction by Suction, A *PARADOX.*

CHAP. I.

I Might, *Sir*, save my self some trouble in giving you that account you desire of me about *Suction*, by referring you to a passage in the *Examen*, I long since writ, of Mr. *Hobbes*'s *Dialogus Physicus de Natura Aeris*, if I knew, you had those two Books lying by you. But because I suspect, that my *Examen*

may not be in your hands, since 'tis almost out of Print, and has not for some years been in my own; and because I do not so well remember, after so long a time, the particulars that I writ there, about Suction, as I do in general, that the *Hypothesis* I proposed, was very incidentally and briefly discours'd of, upon an occasion ministred by a wrong Explication given of Suction by Mr. *Hobbes*, I shall here decline referring you to what I there writ; and proposing to you those thoughts about Suction, that I remember I there pointed at, I shall annex some things to illustrate and confirm them, that would not have been so proper for me to have insisted on in a short and but occasional Excursion.

And I should immediately proceed to what you expect from me, but that *Suction* being generally look'd upon as a kind of *Attraction*, it will be requisite for me to premise something about *Attraction* it self. For, besides that the Cause of it, which

I here dispute not of, is obscure, the very Nature and Notion of it is wont by Naturalists to be either left untouch'd, or but very darkly deliver'd, and therefore will not be unfit to be here somewhat explain'd.

How general and ancient soever the common Opinion may be, that *Attraction* is a kind of Motion quite differing from *Pulsion*, if not also opposite to it; yet I confess, I concur in opinion, though not altogether upon the same grounds, with some modern Naturalists, that think Attraction a *Species* of Pulsion. And at least among inanimate Bodies I have not yet observed any thing, that convinces me, that Attraction cannot be reduced to Pulsion; for, these two seem to me to be but extrinsical denominations of the same Local Motion, in which, if a moved Body *precede* the Movent, or tend to acquire a greater distance from it, we call it *Pulsion*; and if, upon the score of the Motion, the same Body *follow* the Movent or approach to it, we call it

Attraction. But this difference may consist but in an *accidental* respect, which does not *Physically* alter the nature of the Motion, but is founded upon the respect, which the Line, wherein the Motion is made, happens to have to the situation of the Movent. And that which seems to me to have been the chief cause of mens mistaking Attraction for a motion opposite to Pulsion, is, that they have look'd upon both the moving and moved Bodies, in too popular and superficial a manner; and consider'd in the *Movent* rather the situation of the conspicuous and more bulky part of the Animal or other Agent, than the situation of that part of the Animal, or Instrument, that does immediately impress that motion upon the *Mobile*.

For those that attentively heed this, may easily take notice, that some part of that Body, or of the Instrument, which by reason of their conjunction in this operation is to be look'd on but as making one with it, is really placed

placed behind some part of the Body to be drawn, and therefore cannot move outwards it self without thrusting that Body forward. This will be easily understood, if we consider, what happens when a Man draws a Chain after him; for, though his Body do *precede* the Chain, yet his finger or some other part of the hand, wherewith he draws it, has some part or other that reaches *behind* the fore part of the first Link, and the hinder part of this Link comes behind the anteriour part of the second Link, and so each Link has one of its parts placed behind some part of the Link next after it, 'till you come to the last Link of all. And so, as the finger, that is in the first Link, cannot move forwards but it must thrust on that Link, by this *Series* of Trusions the whole Chain is moved forwards, and if any other Body be drawn by that Chain, you may perceive, that some part of the last Link comes behind some part of that Body, or of some intervening Body,

which, by its cohesion with it, ought in our present case to be consider'd as part of it. And thus *Attraction* seems to be but a *Species* of *Pulsion*, and usually belongs to that kind of it, which, for distinctions sake, is called *Trusion*, by which we understand that kind of Pulsion, wherein the Movent goes along with the moved Body without quitting it whilst the progress lasts; as it happens, when a Gardiner drives his Wheel-barrow before him without letting go his hold of it.

But I must not here dissemble a difficulty, that I foresee may be speciously urged against this account of Attraction. For it may be said, that there are Attractions, where it cannot be pretended, that any part of the *Attrahent* comes behind the *Attracted* Body; as in *Magnetical* and *Electrical* Attractions, and in that which is made of Water, when 'tis drawn up into Springs and Pumps.

I need not tell you, that you know so well, as that partly the *Cartesians*, and

and partly other Modern Philoſophers, have recourſe on this occaſion either to *ſcrewed* Particles and other Magnetical Emiſſions, to explicate *Phænomena* of this kind. And, according to ſuch *Hypotheſes*, one may ſay, that many of theſe Magnetical and Electrical *Effluvia* come behind ſome parts of the attracted Bodies, or at leaſt of the little ſolid Particles, that are as it were the Walls of their Pores, or procure ſome diſcuſſion of the Air, that may make it thruſt the Moveable towards the Loadſtone or Amber, *&c.* But if there were none of theſe, nor any other ſubtil Agents that cauſe this Motion by a real, though unperceived, Pulſion; I ſhould make a diſtinction betwixt other Attractions and theſe, which I ſhould then ſtile *Attraction by Inviſibles*. But, whether there be really any ſuch in Nature, and why I ſcruple to admit things ſo hard to be conceived, may be elſewhere conſider'd. And you will, I preſume, the freelier allow me this liberty, if, (ſince

in

in this place 'tis proper to do it,) I shew you, that in the last of the instances I formerly objected (that of the drawing up of Water into the Barrel of a Syringe,) there is no true Attraction of the Liquor made by the external Air. I say then, that by the ascending Rammer, as a part of which I here consider the obtuse end, Plug, or Sucker, there is no Attraction made of the contiguous and subjacent Water, but only there is room made for it, to rise into, without being expos'd to the pressure of the superiour Air. For, if we suppose the whole Rammer to be by Divine Omnipotence annihilated, and consequently uncapable of exercising any Attraction, yet, provided the superiour Air were kept off from the Water by any other way as well as 'twas by the Rammer, the Liquor would as well ascend into the Cavity of the Barrel; since, (as I have elsewhere abundantly proved,) the surface of the Terraqueous Globe being continually pres'd on by the incumbent

bent Air or Atmosphere, the Water must be by that pressure impell'd into any cavity here below, where there is no Air to resist it; as by our Supposition there is not in the Barrel of our Syringe, when the Rammer, or whatever else was in it, had been annihilated. Which Reasoning may be sufficiently confirm'd by an Experiment, whereby I have more than once shewn some curious persons, that, if the external Air, and consequently its pressure, be withdrawn from about the Syringe, one may pull up the Sucker as much as he pleases, without drawing up after it the subjacent Water. In short, let us suppose, that a Man standing in an inner room does by his utmost resistance keep shut a Door, that is neither lock'd nor latch'd, against another, who with equal force endeavours to thrust it open: In this case, *as* if one should forcibly pull away the first Man, it could not be said, that this Man, by his recess from the Door he endeavoured to

press

press outwards, did truely and properly draw in his Antagonist, though upon that recess the coming in of his Antagonist would presently ensue; so it cannot properly be said, that by the ascent of the Rammer, which displaces the superiour Air, either the Rammer it self, or the expelled Air, does properly *attract* the subjacent Water, though the ingress of that Liquor into the Barrel does thereupon necessarily ensue. And that, as the Comparison supposes, there is a pressure of the superiour Air against the upper part of the Sucker, you may easily perceive, if having well stopt the lower orifice of the Syringe with your finger, you forcibly draw up the Sucker to the top of the Barrel. For if then you let go the Rammer, you will find it impell'd downwards by the incumbent Air with a notable force.

CHAP.

Chap. II.

HAving thus premis'd something in general about the Nature of *Attraction*, as far as 'tis necessary for my present design; it will be now seasonable to proceed to the consideration of that kind of Attraction, that is employed to raise Liquors, and is by a distinct Name called *Suction*.

About the Cause of this there is great contention between the New Philosophers, as they are stiled, and the Peripateticks. For the Followers of *Aristotle*, and many Learned Men that in other things dissent from him, ascribe the ascension of Liquors upon *Suction* to Natures abhorrence of a *Vacuum*. For, say they, when a Man dips one end of a Straw or Reed into stagnant Water, and sucks at the other end, the Air contain'd in the cavity of the Reed passes into that

of

of his Lungs, and consequently the Reed would be left empty, if no other Body succeeded in the place it deserts; but there are only (that they take notice of,) two Bodies that can succeed, the Air and the (grosser Liquor) the Water; and the Air cannot do it, because of the interposition of the Water, that denies it access to the immers'd orifice of the Reed, and therefore it must be the Water it self, which accordingly does ascend to prevent a *Vacuum* detested by Nature.

But many of the Modern Philosophers, and generally all the *Corpuscularians*, look upon this *Fuga Vacui* as but an imaginary Cause of *Suction*, though they do it upon very differing grounds. For, the *Atomists*, that willingly admit of Vacuities, properly so called, both within and without our World, cannot think that Nature hates or fears a *Vacuum*, and declines her usual course to prevent it: And the *Cartesians*, though they do, as well as the *Peripateticks*, deny

deny that that there is a *Vacuum*, yet since they affirm not only, that there *is* none *in rerum Natura*, but that there *can be* none, because what others call an empty Space having three Dimensions, hath all that they think belonging to the Essence of a *Body*, they will not grant Nature to be so indiscreet, as to strain her self to prevent the making of a thing that is impossible to be made.

The *Peripatetic* Opinion about the Cause of Suction, though commonly defended by the Schools, as well Modern as Ancient, supposes in Nature such an abhorrence of a *Vacuum*, as neither has been well proved, nor does well agree with the lately discover'd Phænomenon of *Suction*. For, according to their *Hypothesis*, Water and other Liquors should ascend upon Suction to any hight to prevent a *Vacuum*, which yet is not agreeable to experience. For I have carefully tryed, that by pumping with a Pump far more stanch than those that are usually made, and indeed as well

clos'd

clos'd as we could possibly bring it to be, we could not by all our endeavours raise Water by Suction to above *36 ½ foot. The *Torricellian* Exp^t shews, that the weight of the Air is able to sustain, and some of our Experim^ts shew, 'tis able to raise a Mercurial Cylinder equal in weight to as high a Cylinder of Water as we were able to raise by pumping. For Mercury being near 14 times as heavy as Water of the same bulk, if the weight of the Air be equivalent to that of a Mercurial Cylinder of 29 or 30 Inches, it must be able to counterpoise a Cylinder of Water near fourteen times as long, that is, from thirty four to near thirty six foot. And very disagreeable to the common *Hypothesis*, but consonant to ours, is the Experiment that I have more than once tryed, and I think elsewhere deliver'd, namely, That, if you take a Glass Pipe of about three foot long, and, dipping one end of it in Water, suck at the other, the Water will be suddenly made

*See Cont. of *Phys. Mech. Exp.* the 15th *Exp.*

made to flow briskly into your mouth: But, if instead of Water you dip the lower end into Quicksilver, though you suck as strongly as ever you can, provided that in this case, as in the former, you hold the Pipe upright, you will never be able to suck up the Quicksilver near so high as your mouth; so that if the Water ascended upon Suction to the top of the same Pipe, because else there would have been a *Vacuum* left in the cavity of it, why should not we conclude, that, when we have suckt up the Quicksilver as strongly as we can, as much of the upper part of the Tube as is deserted by the Air, and yet not fill'd by the Mercury, admits, in part at least, a *Vacuum*, (as to Air) of which consequently Nature cannot reasonably be suppos'd to have so great and unlimited an abhorrency, as the *Peripateticks* and their Adherents presume. Yet I will not determine, whether there be any more than many little Vacuities, or Spaces devoid of Air, in the Cavity,

so called, of the Pipe unfill'd by the Mercury; (so that the whole Cavity is not one entire empty Space;) it being sufficient for my purpose, that my Experiment affords a good Argument *ad hominem* against the *Peripateticks*, and warrants us to seek for some other Cause than the *fuga Vacui*, why a much stronger Suction than that, which made Water ascend with ease into the Suckers mouth, will not also raise Quicksilver to the same height or near it.

Those Modern Philosophers that admit not the *fuga Vacui* to be the Cause of the raising of Liquors in Suction, do generally enough agree in referring it to the action of the Suckers *thorax*. For, when a Man endeavours to suck up a Liquor, he does by means of the Muscles enlarge the cavity of his Chest, which he cannot do but at the same time he must thrust away those parts of the ambient Air that were contiguous to his Chest, and the displac'd Air does, according to some Learned Men,

(therein,

(therein, if I mistake not, Followers of *Gassendus*,) compress the contiguous Air, and that the next to it, and so outwards; 'till the pressure, successively passing from one part of the Air to the other, arrive at the surface of the Liquor; and all other places being as to sense full, the impell'd Air cannot find place but by thrusting the Water into the room made for it in the Pipe by the recess of the Air that pass'd into the Suckers lungs. And they differ'd not much from this Explication, that, without taking in the compression of the ambient Air made by the *thorax*, refer the Phænomenon to the propagated motion or impulse, that is impress on the Air displac'd by the *thorax* in its dilatation, and yet unable to move in a World perfectly fill'd, as they suppose ours to be, unless the Liquor be impell'd into as much of the cavity of the Pipe, as fast as 'tis deserted by the Air that is said to be suck'd up. But though I readily confess this Explication to be ingenious, and such as I won-

wonder not they ſhould acquieſs in, who are acquainted but with the long known and obvious *Phænomena* of Suction; and though I am not ſure, but that in the moſt familiar caſes the Cauſes aſſign'd by them may contribute to the Effect; yet, preſerving for *Carteſius* and *Gaſſendus* the reſpect I willingly pay ſuch great Philoſophers, I muſt take the liberty to tell you, that I cannot acquieſs in their Theory. For I think, that the Cauſe of *Suction*, they aſſign, is in many caſes not neceſſary, in others, not ſufficient. And *firſt*, as to the Condenſation of the Air by the dilatation of the Suckers Cheſt; when I conſider the extent of the ambient Air, and how ſmall a compreſſion no greater an expanſion than that of the *Thorax* is like to make, I can ſcarce think, ſo ſlight a condenſation of the free Air can have ſo conſiderable an operation on the ſurface of the Liquor to be rais'd, as the *Hypotheſis* I examin requires: And that this impulſe of the Air by a Suckers dilated

dilated *Thorax*, though it be wont to accompany the ascension of the water procured by Suction, yet is not of absolute necessity to it, will, I presume, be easily granted, if it can be made out, that even a propagated *Pulsion*, abstracted from any Condensation of Air, is not so necessarily the Cause of it, but that the Effect may be produc'd without it. For suppose, that by Divine Omnipotence so much Air as is displac'd by the *Thorax* were annihilated; yet I see not, why the Ascension of the Liquor should not ensue. For, when a Man begins to suck, there is an *Æquilibrium*, or rather Æquipollency *between* the pressure, which the Air, contained in the Pipe, (which is shut up with the pressure of the Atmosphere upon it,) has, by virtue of its *Spring*, upon that part of the surface of the water that is environ'd by the sides of the Pipe, *and* the pressure which the Atmospherical Air has, by virtue of its *weight*, upon all the rest of the surface of the stagnant water; so that,

when by the dilatation of the Suckers *Thorax*, the Air within the cavity of the Pipe comes to be rarified, and conſequently looſe of its *Spring*, the *weight* of the external Air continuing in the mean time the ſame, it muſt neceſſarily happen, that the Spring of the internal Air will be too weak to compreſs any longer the gravitation of the external, and conſequently, that part of the ſurface of the ſtagnant water, that is included in the Pipe, being leſs preſs'd upon, than all the other parts of the ſame ſurfaces muſt neceſſarily give way, where it can leaſt reſiſt, and conſequently be impell'd up into the Pipe, where the Air, having had its Spring weakened by expanſion, is no longer able to reſiſt, as it did before. This may be illuſtrated by ſomewhat varying an Inſtance already given, and conceiving, that within a Chamber three Men thruſt all together with their utmoſt force againſt a Door, (which we ſuppoſe to have neither Bolt nor Latch) to keep it ſhut, at

the

the same time the three other Men have just equal strength, and imploy their force to thrust it open. For though, whilst their opposite endeavours are equal, the Door will continue to be kept shut, yet if one of the three Men within the Room should go away, there will need no new force, nor other accession of strength to the three Men, to make them prevail and thrust open the Door against the resistance of those that endeavour'd to keep it shut, who are now but two.

And here (upon the by) you may take notice, that, to raise water in Suction, there is no necessity of any rarified and forcibly stretch'd Rope, as 'twere, of the Air, to draw up the subjacent water into the Pipe, since the bare debilitation of the Spring of the included Air may very well serve the turn. And though, if we should suppose the Air within the Pipe to be quite annihilated, it could not be pretended (since it would not have so much as Existence) that it exer-

cises an attractive Power; yet in this case the water would ascend into the Pipe, without the assistance of Natures imaginary Abhorrence of a *Vacuum*, but by a Mechanical Necessity, plainly arising from this, that there would be a pressure of the incumbent Atmosphere upon the rest of the surface of the stagnant water, and no pressure at all upon that part of the surface that is within the Pipe, where consequently there could be no resistance made to the ascension of the water, every where else strongly urg'd by the weight of the incumbent Air.

I shall add on this occasion, that, to shew some inquisitive Men, that the weak resistance within a Vessel, that had but one orifice expos'd to the water, may much more contribute to the ascension of that Liquor into the Vessel, than either the compression or the continued or reflected impulse of the external Air; I thought fit to produce a Phænomenon, which by the Beholders was without scruple judg'd

judg'd an Effect of Suction, and yet could not be aſcrib'd to the Cauſe of Suction, aſſign'd by either of the Sects of Philoſophers I diſſent from. The Experiment was this: By a way, elſewhere deliver'd, the long neck of a Glaſs-bubble was ſeal'd up, and almoſt all the Air had been by Heat driven out of the whole cavity of the Bubble or Vial, and then the Glaſs was laid aſide for ſome hours, or as long as we pleas'd; afterwards the ſeal'd *apex* of the neck was broken off under water: I demand now of a *Peripatetic*, whether the Liquor ought to be ſuck'd or drawn into the cavity of the Glaſs, and why? if he ſays, as queſtionleſs he will, that the water would be attracted to hinder a *Vacuum*, he would thereby acknowledge, that, 'till the Glaſs was unſtopt under water, there was ſome empty ſpace in it; for, 'till the ſealed end was broken off, the water could not get in, and therefore, if the *fuga vacui* had any thing to do in the aſcenſion, the Liquor muſt riſe, not to pre-

prevent an empty space, but to fill one that was made before. Nor does our Experiment much more favour the other Philosophers, I dissent from: For in it there is no dilatation made of the sides of the Glass, as in ordinary Suction there is made of the *Thorax*, but only there is so much Air driven out of the cavity of the Bubble, into whose room since neither common Air nor Water is permitted to succeed, it appears not, how the propagated and returning impulse, or the Circle of Motion, as to common Air and Water, does here take place. And then I demand, what becomes of the Air, that has been by heat driven out, and is by the Hermetical Seal kept out of the cavity of the Bubble? If it be said, that it diffuses it self into the ambient Air, and mingles with it, that will be granted which I contended for, that so little Air as is usually displac'd in Suction cannot make any considerable compression of the free ambient Air; for, what can one Cubic Inch of

of Air, which is sometimes more than one of our Glasses contains, do, to the condensation so much as of all the Air in the Chamber, when the expell'd Corpuscles are evenly distributed among those of the ambient. And how comes this inconsiderable condensation to have so great an effect in every part of the room, as to be able there to impel into the Glass as much water in extent as the whole Air that was driven out of the cavity of it? But if it be said, that the expell'd Air condens'd only the contiguous or very neighbouring Air, 'tis easie to answer, that 'tis no way probable, that the expell'd Particles of the Air should not by the differing motions of the ambient Air be quickly made to mingle with it, but should rather wait (which if it did we sometimes made it do for many hours) 'till the Vessels whence 'twas driven out were unstopp'd again. But, though this could probably be pretended, it cannot truly be asserted. For if you carry the seal'd Glass quite out of the

room

room or house, and unstop it at some other place, though two or three miles distant; the ascension of the water will, (as I found by tryal) nevertheless insue; in which case I presume, it will not be said, that the Air, that was expell'd out of the Glass, and condens'd the contiguous or near contiguous Air, attended the Bubble in all its motions, and was ready at hand to impel in the water, as soon as the seal'd *apex* of the Vial was broken off. But I doubt not, but most of the Embracers of the Opinion I oppose, being Learned and Ingenuous Persons, if they had been acquainted with these and the like *Phænomena*, would rather have changed their Opinion about *Suction*, than have gone about to defend it by such Evasions, which I should not have thought worth proposing, if I had not met with Objections of this nature publickly maintain'd by a Learned Writer, on occasion of the Air's rushing into the exhausted *Magdenburgic* Engine. But as in our Experiment

riment these Objections have no place, so in our *Hypothesis* the Explication is very easie, as will anon be intimated.

Chap. III.

Having thus shewn, that the Ascension of Water upon Suction may be caus'd otherwise than by the Condensation or the propagated Pulsion of Air contiguous to the Suckers *Thorax*, and thrust out of place by it; it remains that I shew, (which was one of the two things I chiefly intended,) that there may be Cases wherein the Cause, assign'd in the *Hypothesis* I am examining, will not have place. But this will be better understood, if, before I proceed to the proof of it, I propose to you the thoughts, I had many years since, and do still retain, about the Cause of the Ascension of Liquors in Suction.

To clear the way to the right understanding

derstanding of the ensuing Discourse, it will not be amiss here to premise a summary intimation of some things that are suppos'd in our *Hypothesis*.

We suppose then *first*, without disputing either the Existence or the nature of Elementary Air, that the Common Air we breath in, and which I often call Atmospherical Air, abounds with Corpuscles not devoid of Weight, and indowed with Elasticity or Springiness, whereby the lower parts, compress't by the weight of the upper, incessantly endeavour to expand themselves, by which expansion, and in proportion to it, the Spring of the Air is weaken'd, (as other Springs are wont to be) the more they are permitted to stretch themselves.

Next, we suppose, that the Terraqueous Globe, being inviron'd with this gravitating and springy Air, has its surface and the Bodies plac'd on it prest by as much of the Atmosphere as either perpendicularly leans on them, or can otherwise come to bear upon

upon them. And this preſſure is by the *Torricellian* and other Experiments found to be equivalent to a perpendicularly erected Cylinder of about twenty nine or thirty Inches of Quickſilver, (for the height is differing, as the gravity of the Atmoſphere happens to be various.)

Laſtly, we ſuppoſe, that, Air being contain'd in a Pipe or other hollow Body that has but one orifice open to the free Air, if this orifice be Hermetically ſeal'd, or otherwiſe (as with the mouth of one that ſucks) clos'd, the now included Air, whilſt it continues without any farther expanſion, will have an elaſticity equivalent to the weight of as much of the outward Air as did before preſs againſt it. For, if the weight of the Atmoſphere, to which it was then expos'd, had been able to compreſs it further, it would have done ſo, and then the cloſing of the orifice, at which the internal and external Air communicated, as it fenc'd the included Air from the preſſure

of

of the incumbent, so it hindred the same included Air from expanding it self; so that, as it was shut up with the pressure of the Atmosphere upon it, that is in a state of as great compression as the weight of the Atmosphere could bring it to, so, being shut up and thereby kept from weakening that pressure by expansion, it must retain a Springiness equipollent to the pressure 'twas expos'd to before, which (as I just now noted) was as great as the weight of the incumbent Pillar of the Atmosphere could make it. But if, as was said in the first Supposition, the included Air should come to be dilated or expanded, the Spring being then unbent, its Spring, like that of other elastical Bodies, would be debilitated answerably to that expansion.

To me then it seems, that, speaking in general, Liquors are upon Suction raised into the cavities of Pipes and other hollow Bodies, when, and so far as, there is a less pressure on

on the surface of the Liquor in the cavity, than on the surface of the external Liquor that surrounds the Pipe, *whether* that pressure on those parts of the external Liquor, that are from time to time impell'd up into the orifice of the Pipe, proceed from the weight of the Atmosphere, *or* the propagated compression or impulse of some parts of the Air, *or* the Spring of the Air, *or* some other Cause, as the pressure of some other Body quite distinct from Air.

Upon the general view of this *Hypothesis*, it seems very consonant to the Mechanical Principles. For, if there be on the differing parts of the surface of a fluid Body unequal pressures, 'tis plain, as well by the nature of the thing, as by what has been demonstrated by *Archimedes*, and his Commentators, that the greater force will prevail against the lesser, and that that part of the waters surface must give way, where it is least prest. So that that, wherein the *Hypothesis* I venture to propose to you,

differs from that which I dissent from, is not; that mine is less Mechanical; but *partly* in this, that, whereas the *Hypothesis*, I question, supposes a necessity of the protrusion or impulse of the Air, mine does not require that supposition, but, being more general, reaches to other ways of procuring the Ascension of Liquors, without raising them by the impulse of the Air; and *partly*, and indeed chiefly, in that the *Hypothesis*, I decline, makes the Cause of the Ascension of Liquors to be only the increased pressure of the Air external to the pipe; and I chiefly make it to depend upon the diminished pressure of the Air within the pipe, on the score of the expansion 'tis brought to by Suction.

To proceed now to some Experiments that I made in favour of this *Hypothesis*, I shall begin with that which follows:

We took a Glass-pipe bended like a Syphon, but so that the shorter legg was as parallel to the longer as we could

could get it made, and was Hermetically seal'd at the end: Into this Syphon we made a shift (for 'tis not very easie) to convey water, so that the crooked part being held downwards, the liquor reach'd to the same height in both the leggs, and yet there was about an Inch and half of uncompress Air shut up in the shorter legg. This little Instrument (for 'twas but about fifteen Inches long) being thus prepar'd, 'tis plain, that according to the *Hypothesis* I dissent from, there is no reason, why the water should ascend upon Suction. For, though we should admit, that the external Air were considerably compress, or received a notable impulse, when the Suckers chest is enlarged; yet in our case that compression or protrusion will not reach the surface of the water in the shorter legg, because it is there fenc'd from the action of the external Air by the sides of the Glass, and the Hermetical Seal at the top. And yet, if one suck'd strongly at the open orifice in the

longer legg, the water in the ſhorter would be depreſt; and that in the longer aſcended at one ſuck about an Inch and half: Of which the reaſon is clear in our *Hypotheſis*. For, the Spring of the included Air, together with the weight of the water in the ſhorter legg, and the preſſure of the Atmoſpherical Air, aſſiſted by the weight of the liquor in the longer legg, counter-ballanced one another before the Suction began: But, when afterwards upon Suction the Air in the longer legg came to be dilated and thereby weaken'd, 'twas render'd unable to reſiſt the undiminiſh'd preſſure of the Air included in the ſhorter legg, which conſequently expanding it ſelf by vertue of its Elaſticity, depreſt the contiguous water, and made it proportionably riſe in the oppoſite legg, 'till by the expanſion its Spring being more and more weaken'd, it arrived at an equipollency with the gravitation or preſſure of the Atmoſphere. Which laſt clauſe contains the Reaſon, why, when the

the perſon that ſuckt had rais'd the water in the longer legg *leſs than* three Inches higher by repeated endeavours to ſuck, and that without once ſuffering the water to fall back again, he was not able to elevate the water in the longer, *ſo much as* three Inches above its firſt ſtation. And if in the ſhorter legg there was but an Inch and a quarter of ſpace left for the Air unfill'd by the water, by divers skilfully reiterated acts of Suction he could not raiſe the liquor in the longer legg above two Inches; becauſe by that time the Air included in the ſhorter legg had, by expanding it ſelf further and further, proportionably weaken'd its Spring, 'till at length it became as rarified, as was the Air in the cavity of the longer legg, and conſequently was able to thruſt away the water with no more force than the Air in the long legg was able to reſiſt. And by the recited tryal it appear'd, that the rarefaction uſually made of Air by Suction is not near ſo great, as one would

expect, problably becauſe by the dilatation of the Lungs the Air, being ſtill ſhut up, is but moderately rarified, and the Air in the longer legg can by them be brought to no greater degree of rarity, than that of the Air within the Cheſt. For, whereas the included Air in our Inſtrument was not expanded, by my eſtimate, at one ſuck to above the double of its former dimenſions, and by divers ſucceſſive ſucks was expanded but from one Inch and an half to leſs than four Inches and an half, if the Suction could have been conveniently made with a great and ſtanch Syringe, the rarefaction of the Air would probably have been far greater; ſince in our Pneumatick Engin Air may, without heat, and by a kind of Suction, be brought to poſſeſs many hundreds of times the ſpace it took up before. From this raréfaction of the Air in both the leggs of our Inſtrument proceeds another *Phænomenon*, readily explicable by our *Hypotheſis*. For if, when the water was impell'd

impell'd up as high as the Suction could raise it, the Instrument were taken from the Suckers mouth, the elevated water would with violence return to its wonted station. For, the Air, in both the leggs of the Instrument, having by the Suction lost much of the Spring, and so of its power of pressing; when once the orifice of the longer legg was left open, the Atmospherical Air came again to gravitate upon the water in that legg, and the Air, included in the other legg, having its Spring debilitated by the precedent expansion, was not able to hinder the external Air from violently repelling the elevated water, 'till the included Air was thrust into the space it possess'd before the Suction; in which space it had Density and Elasticity enough to resist the pressure, that the external Air exercis'd against it through the interpos'd water.

But our *Hypothesis* about the Cause of Suction would not need to be solicitously prov'd to you by other ways,

if you had seen what I have sometimes been able to do in our Pneumatick Engin. For, there we found by tryals purposely devis'd, and carefully made, that a good Syringe being so conveyed into our Receiver, that the open orifice of the Pipe or lower part was kept under water, if the Engin were exhausted, though the handle of the Syringe were drawn up, the water would not follow it, which yet it would do if the external Air were let in again. The Reason of which is plain in our *Hypothesis*. For, the Air, that should have prest upon the surface of the stagnant water, having been pumpt out, there was nothing to impell up the water into the deserted cavity of the Syringe, as there was when the Receiver was fill'd with Air.

CHAP.

Chap. IV.

But because such a conveniency as our Engin, and the *apparatus* necessary for such Tryals are not easily procurable, I shall endeavour to confirm our *Hypothesis* about Suction by subjoining some Experiments, that may be tryed without the help of that Engin, for the making out these three things:

I. *That a Liquor may be rais'd by Suction, when the pressure of the Air, neither as it has Weight nor Elasticity, is the Cause of the Elevation.*

II. *That the weight of the Atmospherical Air is sufficient to raise up Liquors in Suction.*

III. *That in some cases Suction will not be made, as, according to the Hypothesis I dissent from, it should, although there be a dilatation of the Suckers* Thorax, *and no danger of a* Vacuum *though the Liquor should ascend.*

And

And *first*, to shew, how much the rising of Liquors in Suction depends upon the weight or pressure of the impellent Body, and how little necessity there is, where that pressure is not wanting, that, in the place deserted by the Liquor that is suck'd, there should succeed Air or some other visible Body, as the *Peripatetic* Schools would have it; to shew this, *I say*, I thought on the following Experiments. We took a Glass-pipe fit to have the *Torricellian* Experiment made with it, but a good deal longer than was necessary for that use: This Pipe being Hermetically seal'd at one end, the other end was so bent as to be reflected upwards, and make as it were the shorter legg of the Syphon as parallel as we could to the longer, so that the Tube now was shap'd like an inverted Syphon with leggs of a very unequal length. This Tube, notwithstanding its inconvenient figure, we made a shift, (for 'tis not easily done) to fill with Mercury, when 'twas in an inclin'd posture,

posture, and then erecting it; the Mercury subsided in the longer legg, as in the *Torricellian* Experiment, and attain'd to between two foot and a quarter and two foot and an half above the surface of the Mercury in the shorter legg, which in this Instrument answers to the stagnant Mercury in an ordinary *Barometer*, from which to distinguish it I have elsewhere call'd this Syphon, furnish'd with Mercury, a *Travelling Baroscope*, because it may be safely carried from place to place. Out of the shorter legg of this Tube we warily took as much Mercury as was thought convenient for what we had further to do, and this we did by such a way as to hinder any Air from getting into the deserted cavity of the longer legg, by which means the Mercurial Cylinder, (estimated as I lately mention'd) retain'd the same height above the stagnant Mercury in the shorter: The upper and clos'd part of this Travelling Baroscope you will easily grant to have been free from Common

Air,

Air, not only for other Reasons that have been given elsewhere, but particularly for this, that, if you gently incline the Instrument, the Quicksilver will ascend to the top of the Tube; which you know it could not do, if the place, formerly deserted by it, were possest by the Air, which by its Spring would hinder the ascension of the Mercury, (as is easie to be tryed.) The Instrument having been thus fitted, I caus'd one of the bystanders to suck at the shorter legg, whereupon (as I expected) there presently ensued an Ascension of four or five Inches of Mercury in that legg, and a proportionable Subsidence of the Mercury in the longer, and yet in this case the raising of the Mercury cannot be pretended to proceed from the pressure of the Air. For, the weight of the Atmosphere is fenc'd off by that, which closes the upper end of the longer Tube, and the Spring of the Air has here nothing to do, since, as we have lately shewn, the space deserted by the Mercury is

not

not poſſeſt by the included Air, and the pulſion or condenſation of the Air, ſuppos'd by divers modern Philoſophers to be made by the dilatation of the Suckers Cheſt, and to preſs upon the ſurface of the Liquors that are to be ſuck'd up; this, *I ſay*, cannot here be pretended in regard the ſurface of the Liquor in the longer legg is every way fenc'd from the preſſure of the ambient Air. So that it remains, that the Cauſe, which rais'd the Quickſilver in the ſhorter legg upon the newly recited Suction, was the weight of the collaterally ſuperiour Quickſilver in the longer legg, which, being (at the beginning of the Suction) equivalent to the weight of the Atmoſphere, there is a plain reaſon, why the ſtagnant Mercury in the ſhorter legg ſhould be rais'd ſome Inches by Suction; as Mercury ſtagnant in an open Veſſel will be rais'd by the weight of the Atmoſphere, when the Suction is made in the open Air. For, in both caſes there is a Pipe, that reaches to the ſtagnant Mercury,

Mercury, and a competent weight to impel it into that Pipe; when the Air in the cavity of the Pipe has its Spring weaken'd by the dilatation that accompanied Suction.

The *Second* point formerly propos'd, which is, *That the weight of the Air is sufficient to raise Liquors in Suction*; may not be ill prov'd by Arguments legitimately drawn from the *Torricellian* Experiment it self, and much more clearly by the first and fifteenth of our *Continued Physico-Mechanical Experiments*. And therefore I shall only here take notice of a Phænomenon, that may be exhibited by the *Travelling Baroscope*, which, though it be much inferiour to the Experiments newly referr'd to, may be of some use on the present occasion.

Having then provided an Instrument like the *Travelling Baroscope*, mention'd under the former Head, but whose leggs were not so unequally long, and having in it made the *Torricellian* Experiment after the manner lately describ'd; we order'd the matter

ter ſo, that there remain'd in the ſhorter legg the length of divers Inches unfill'd with ſtagnant Mercury. Then I caus'd one, vers'd in what he was to do, ſo to raiſe the Quick-ſilver by Suction to the open orifice of the ſhorter legg, that, the orifice being ſeaſonably and dexterouſly cloſed, the Mercury continued to fill that legg, as long as we thought fit; and then having put a mark to the ſurface of the Mercury in the longer legg, we unſtopp'd the orifice of the ſhorter; whereupon the Mercury, that before fill'd it, was *depreſs'd*, 'till the ſame Liquor in the longer legg was rais'd five Inches or more above the mark, and continu'd at that height. I ſaid, that the Mercury that had been raiſed by Suction, was *depreſs'd*, rather than that it *ſubſided*, becauſe its own weight could not here make it fall, ſince a Mercurial Cylinder of five Inches was far from being able to raiſe ſo tall a Cylinder of Mercury as made a counterpoiſe in the longer legg; and therefore the depreſſion we ſpeak of,

of, is to be referr'd to the gravitation of the Atmoſpherical Air upon the ſurface of the Mercury in the ſhorter legg: And I ſee no cauſe to doubt but that, if we could have procured an Inſtrument, into whoſe ſhorter legg a Mercurial Cylinder of many Inches higher could have been ſuck'd up, it would by this contrivance have appear'd, that the preſſure of the Atmoſphere would eaſily impel up a far taller Cylinder of Mercury than it did in our recited Experiment.

That this is no groundleſs conjecture may appear probable by the Experiment you will preſently meet with. For if the gravity of an incumbent Pillar of the Atmoſphere be able to compreſs a parcel of included Air as much as a Mercurial Cylinder, equivalent in weight to between thirty and five and thirty foot of water, is able to condenſe it, it cannot well be denied that the ſame Atmoſpherical Cylinder may be able by its weight to raiſe and counterballance

ballance eight or nine and twenty Inches of Quicksilver, or an equivalent pillar of water in Tubes, where the resistance of these two Liquors to be rais'd and sustain'd by the Air, depends only upon their own unassisted gravity.

To confirm our Doctrine of the Gravitation of the Atmosphere upon the surface of the Liquors expos'd to it, I will subjoin an Experiment, that I devis'd to shew, that the incumbent Air, in its natural or usual state, would compress other Air not rarified, but in the like natural state, as much as a Cylinder of eight or nine and twenty Inches of Mercury would condense or compress it.

In order to the making of this, I must put you in mind of what I have shewn elsewhere at large, and shall further confirm by one of the Experiments that follows the next; namely, that about twenty nine or thirty Inches of Quicksilver

See the Authors Defence of the Doctrine touching the Spring and Weight of the Air, against Fr. Linus, *chap.* 5.

will compreſs Air, that being in its natural or uſual ſtate (as to rarity and denſity) has been ſhut up in the ſhorter legg of our Travelling or Syphon-like Baroſcope, into half the room that included Air poſſeſs'd before. This premis'd, I paſs on to my Experiment, which was this:

We provided a *Travelling Baroſcope*, wherein the Mercury in the longer legg was kept ſuſpended by the counterpoiſe of the Air that gravitated on the ſurface of the Mercury in the ſhorter legg, which we had ſo order'd, that it reached not by about two Inches to the top of the ſhorter legg. Then making a mark at the place where the ſtagnant Mercury reſted, 'twas manifeſt according to our *Hypotheſis*, that the Air in the upper part of the ſhorter legg was in its natural ſtate, or of the ſame degree of denſity with the outward Air, with which it freely communicated at the open orifice of the ſhorter legg; ſo that this ſtagnant Air was equally preſt upon by the weight of

of the collaterally ſuperiour Cylinder of Mercury in the longer legg, and the equivalent weight of a directly incumbent pillar of the Atmoſphere. Things being in this poſture, the upper part of the ſhorter legg, which had been before purpoſely drawn out to an almoſt capillary ſmallneſs, was Hermetically ſeal'd, which, though the Inſtrument was kept erected, was ſo nimbly done by reaſon of the ſlenderneſs of the Pipe, that the included Air did not appear to be ſenſibly heated, though for greater caution we ſtaid a while from proceeding, that, if any rarefaction had been produc'd in the Air, it might have time to loſe it again. This done, we open'd the lower end of the longer legg, (which had been ſo order'd before, that we could eaſily do it, and without concuſſion of the Veſſel,) by which means the Atmoſpherical Air, gaining acceſs to the Mercury included in the longer legg, did, as I expected, by its gravitation upon it ſo compreſs the Air included in

the shorter legg, that, according to the estimate we made with the help of a Ruler, (for by reason of the conical figure of the upper part of the glass we could not take precise measures,) it was thrust into near half the room it took up before, and consequently, according to what I put you lately in mind of, endur'd a compression like that, which a Mercurial Cylinder of about twenty nine Inches would have given it.

This Experiment, as to the main of it, was for greater caution made the second time with much the like success; and though it had been more easie to measure the Condensation of the Air, if, instead of drawing out and sealing up the shorter legg of the Instrument, we had contented our selves to close it some other way; yet we rather chose to imploy *Hermes*'s Seal, lest, if any other course had been taken, it might be pretended, that some of the included Air, when it began to be compress't, might escape out at the not perfectly and strongly clos'd

clos'd orifice of the legg wherein 'twas impriſon'd.

To make it yet further appear, how much the Aſcenſion of Liquors by Suction depends upon Preſſure, rather than upon Natures imaginary Abhorrence of a *Vacuum*, or the propagated Pulſion of the Air; I will ſubjoin an *Inſtance*, wherein that preſum'd Abhorrence cannot be pretended. The Experiment was thus made:

A Glaſs-Syphon, like thoſe lately deſcrib'd, with one legg far longer than the other, was Hermetically ſeal'd at the ſhorter legg, and then by degrees there was put in, at the orifice of the longer legg, as much Quick-ſilver as by its weight ſuffic'd to compreſs the Air in the ſhorter legg into about half the room it poſſeſs'd before; ſo that, according to the *Peripatetick* Doctrine, the Air muſt be in a ſtate of preternatural Condenſation, and that to a far greater degree, than (as I have tryed) 'tis uſually brought to by Cold, intenſe enough to freeze water. Then meaſuring

the heighth of the Quickſilver in the longer Tube above the ſuperficies of that in the ſhorter, we found it not exceed thirty Inches. Now, if Liquors did riſe in Suction *ob fugam vacui*, there is no reaſon, why this Quickſilver in the longer part of the Syphon ſhould not eaſily aſcend upon Suction, at leaſt 'till the Air in the ſhorter legg had regain'd its former Dimenſions, ſince it cannot in this place be pretended, that, if the Mercury ſhould aſcend, there would be any danger of a *Vacuum* in the ſhorter legg of the Tube, in regard that the contiguous included Air is ready at hand to ſucceed as faſt as the Mercury ſubſides in the ſhorter legg of the Syphon. Nor can it be pretended, that, to fill the place deſerted by the Quickſilver, the included Air muſt ſuffer a preternatural rarefaction or diſcenſion; ſince 'tis plain in our caſe, that on the contrary, as long as the Air continues in the ſtate whereto the weight of the Quickſilver has reduc'd it, it is kept in a violent ſtate

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of compreſſion; ſince in the ſhorter legg it was in its natural ſtate, when the Mercury, poured into the longer legg, did by its weight thruſt it into about half the room it took up before. And yet, having caus'd ſeveral perſons, one of them vers'd in ſucking, to ſuck divers times as ſtrongly as they could, they were neither of them able, not ſo much as for a minute of an hour, to raiſe the Mercury in the longer legg, and make it ſubſide in the ſhorter for more than about an Inch at moſt. And yet to ſhew you, that the Experiment was not favourably tryed for me, the height of the Mercurial Cylinder in the longer legg above the ſurface of that in the ſhorter legg was, when the Suction was tryed, an Inch or two ſhorter than thirty Inches, and the compreſt Air in the ſhorter legg was ſo far from having been by the exſuction expanded beyond its natural and firſt dimenſions, that it did not, when the contiguous Mercury ſtood as low as we could

make it ſubſide, regain ſo much as one half of the ſpace it had loſt by the precedent Compreſſion, and conſequently was in a preternatural ſtate of condenſation, when it had been freed from that ſtate as far as Suction would do it. Whence it ſeems evident, that 'twas not *ob fugam vacui*, that the Quickſilver did upon Suction aſcend one Inch; for, upon the ſame ſcore it ought to have aſcended two, or perhaps more Inches, ſince there was no danger, that by ſuch an aſcenſion any *Vacuum* ſhould be produc'd or left in the ſhorter legg of the Syphon; whereas, according to our *Hypotheſis*, a clear cauſe of the Phænomenon is aſſignable. For, before the Suction was begun, there was an *Æquilibrium* or equipollency between the weight of the ſuperiour Quickſilver in the longer legg, and a Spring of the compreſt Air included in the ſhorter legg: But when the Experimentor began to ſuck, his Cheſt being widen'd, part of the Air included in the upper part of the longer

longer legg pass'd into it, and that which remain'd had by that expansion its pressure so weaken'd, that the Air in the shorter legg, finding no longer the former resistance, was able by its own Spring to expand it self, and consequently to depress the contiguous Mercury in the same shorter legg, and raise it as much in the longer.

But here a *Hydrostatician*, that heedfully marks this Experiment, may discern a difficulty, that may perhaps somewhat perplex him, and seems to overthrow our Explication of the Phænomenon. For he may object, that if the compress Air in the shorter legg had a Spring equipollent to the weight of the Mercury in the longer legg, it appears not, why the Mercury should not be suckt up in this Instrument, as well as in the free Air; since, according to me, the pressure of the included Air upon the subjacent Mercury must be equivalent to the weight of the Atmosphere, and yet experience shews, that the

weight

weight of the Atmosphere will, upon Suction, raise Quicksilver to the height of several Inches.

To clear this difficulty, and shew, that, though it be considerable, 'tis not at all insuperable, be pleased to consider with me, that I make indeed the *Spring* of the compress Air to be equipollent to the *Weight* of the compressing Mercury, and I have a manifest reason to do it; because, if the Spring of the Air were not equipollent to that Weight, the Mercury must necessarily compress the Air farther, which 'tis granted *de facto* not to do. But then I consider, that in our case there ought to be a great deal of difference between the operation of the *Spring* of the *included Air* and the *Weight* of the *Atmosphere*, after Suction has been once begun. For, the Weight of the Atmosphere, that impels up Mercury and other Liquors, when the Suction is made in the open Air, continues still the same, but the force or pressure of the included Air is equal to the counterpressure

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of the Mercury no longer than the first moment of the Suction; after which, the force of the imprison'd Air still decreases more and more, since this compress Air, being further and further expanded, must needs have its Spring proportionably weaken'd; so that it need be no wonder, that the Mercury was not suckt up any more than we have related; for there was nothing to make it ascend to a greater height, than that, at which the debilitated Spring of the (included but) expanded Air was brought to an equipollency with the undiminish'd and indeed somewhat increas'd weight of the Mercurial Cylinder in the longer legg, and the pressure of the Aerial Cylinder in the same legg, lessen'd by the action of him that suck'd. For whereas, when the orifice of this legg stood open, the Mercury was prest on by a Cylinder of the Atmospherical Air, equivalent to about thirty Inches of Quicksilver; by the mouth and action of him that suck'd the Tube was freed

freed from the external Air, and by the dilatation of his *Thorax*, the neighbouring Air, that had a free passage through his wind-pipe to it, was proportionably expanded, and had its Spring and pressure weaken'd: By which means, the compress Air in the shorter legg of the Syphon was inabled to impel up the Mercury, 'till the lately mention'd *Equilibrium* or equipollency was attain'd. And I must here take notice, that, as the Quicksilver was rais'd by Suction but a little way, so the Cylinder that was rais'd was a very long one; whereas, when Mercury is suck'd up in the free Air, it is seldom rais'd to half that length; though, as I noted before, the impellent cause, which is the weight of the Atmosphere, continued still the same, whereas in our Syphon, when the Mercury was suck'd up but an Inch, the compress Air, possessing double the space it did before, had by this expansion already lost a very considerable part of its former Spring and Pressure.

I should here conclude this Discourse, but that I remember a *Phænomenon* of our Pneumatic Engin, which to divers Learned Men, especially *Aristotelians*, seem'd so much to argue, that Suction is made either by a *Fuga Vacui*, or some internal Principle, that divers years ago I thought fit to set down another account of it, and lately meeting with that account among other papers, I shall subjoin it just as I found it, by way of *Appendix* to the foregoing Tract.

Among the more familiar *Phænomena* of the *Machina Boyliana*, (as they now call it,) none leaves so much scruple in the Minds of some sorts of Men, as this, That, when ones finger is laid close upon the orifice of the little Pipe, by which the Air is wont to pass from the Receiver into the exhausted Cylinder, the pulp of the finger is made to enter a good way into the cavity of the Pipe, which doth not happen without a considerable sense of pain in the lower part

part of the finger. For most of those that are strangers to *Hydrostaticks*, especially if they be prepossess'd with the Opinions generally receiv'd both in the *Peripatetick* and other Schools, perswade themselves, that they feel the newly mention'd and painful protuberance of the pulp of the finger, to be effected not by pressure, as we would have it, but distinctly by Attraction.

To this we are wont to answer, That common Air being a Body not devoid of weight, the Phenomenon is clearly explicable by the pressure of it: For, when the finger is first laid upon the orifice of the Pipe, no pain nor swelling is produc'd, because the Air which is in the Pipe presses as well against that part of the finger which covereth the orifice, as the ambient Air doth against the other parts of the same finger. But when by pumping, the Air in the Pipe, or the most part of it, is made to pass out of the Pipe into the exhausted Cylinder, then there is nothing left

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in the Pipe, whose pressure can any thing near countervail the undiminish'd pressure of the external Air on the other parts of the finger; and consequently, that Air thrusts the most yielding and fleshy part of the finger, which is the pulp, into that place where its pressure is unresisted, that is, into the cavity of the Pipe, where this forcible intrusion causeth a pain in those tender parts of the finger.

To give some visible Illustration of what we have been saying, as well as for other purposes, I thought on the following Experiment.

We took a Glass-pipe of a convenient length, and open at both ends, whose cavity was near about an Inch in *Diameter*, (such a determinate breadth being convenient, though not necessary:) To one of the ends of this Pipe we caused to be firmly tyed on a piece of very fine Bladder, that had been ruffled and oyl'd, to make it both very limber and unapt to admit water; and care was taken, that the

the piece of Bladder tyed on should be large enough, not only to cover the orifice, but to hang loose somewhat beneath it.

This done, we put the cover'd end of the Pipe into a Glass-body (or Cucurbit) purposely made more than ordinarily tall, and the Pipe being held in such manner, as that the end of it reach'd almost, but not quite, to the bottom of the Glass-body; we caused water to be poured both into this Vessel and into the Pipe (at its upper orifice, which was left open) that the water might ascend equally enough, both without and within the Pipe. And when the Glass-body was full of water, and the same liquor was level to it, or a little higher within the Pipe, the Bladder at the lower orifice was kept plump, because the water within the Pipe did by its weight press as forcibly downwards, as the external water in the large Glass endeavour'd to press it inwards and upwards.

All this being done, we caus'd

part

part of the water in the Pipe to be taken out of it, (which may be done either by putting in and drawing out a piece of Spunge or of Linnen, or more expeditiously by sucking up part of the water with a smaller Pipe to be immediately after laid aside;) upon which removal of part of the internal water, that which remained in the Pipe being no longer able, by reason of its want of weight, to press against the inside of the Bladder near as forcibly as it did before, the external water, whose weight was not lessen'd, pres's'd the sides and bottom of the Bladder, whereto it was contiguous, into the cavity of the Pipe, and thrusted it up therein so strongly, that the distended Bladder made a kind of either Thimble or Hemisphere within the Pipe. So that here we have a protuberance, like that above-mentioned of the finger, effected by Pulsion, not Attraction; and in a case where there can be no just pretence of having recourse to Natures Abhorrence of a

Vacuum, ſince, the upper orifice of the Pipe being left wide open, the Air may paſs in and out without reſiſtance.

The like ſwelling of the Bladder in the Pipe we could procure without taking out any of the internal liquor, by thruſting the Pipe deeper into the water; for then the external liquor, having by reaſon of its increaſe of depth a greater preſſure on the outſide of the Bladder, than the internal liquor had on the inſide of it, the Bladder muſt yield to the ſtronger preſſure, and conſequently be impell'd up.

If the Bladder lying looſe at the lower end of the Pipe, the upper end were carefully clos'd with ones thumb, that the upper Air might not get out until the Experimentor thought fit, and if the thus clos'd Pipe were thruſt almoſt to the bottom of the water, the Bladder would not be protuberant inwards, as formerly; becauſe the included Air by virtue of its Spring, reſiſted from within

within the pressure of the external water against the outside of the Bladder: But if the thumb, that stopp'd the Pipes upper orifice, were remov'd, the formerly compress'd Air having liberty to expand it self, and its elasticity being weaken'd thereby, the external water would with suddenness and noise enough, not to be unpleasant to the Spectators, drive up the Bladder into the cavity of the Pipe, and keep it there very protuberant.

To obviate an Objection, that I foresaw might be brought in by persons not well vers'd in *Hydrostaticks*, I caus'd the Pipe fore-mention'd, or such another, to be so bent near the lower end, as that the orifice of it stood quite on one side, and the parts of the Pipe made an angle as near to a right one as he that blew it could bring it to. This lower orifice being fitted with a Bladder, and the Pipe with its contained liquor being thrust under water after the former manner, the lateral pressure

of the water forc'd the Bladder into the ſhort and horizontal legg, and made it protuberate there, as it had done when the Pipe was ſtraight.

Laſtly, that the Experiment might appear not to be confin'd to one liquor; inſtead of *Water* we put into the unbent Pipe as much red *Wine* (whoſe colour would make it conſpicuous) as was requiſit to keep the Bladder ſomewhat ſwelling outwards, when it was ſomewhat near the bottom of the water; and then 'twas manifeſt, that, according as we had foreſeen, the ſuperficies of the red liquor in the Pipe was a good deal higher than that of the external water, and if the depth of both liquors were proportionably leſſen'd, the difference of height betwixt the two ſurfaces would indeed, as it ought to happen, decreaſe, but ſtill the ſurface of the wine would be the higher of the two, becauſe being lighter in *ſpecie* than the common water, the *Æquilibrium* between the preſſures of the two liquors upon the Bladder would

would not be maintain'd, unleſs a greater height of *wine* compenſated its defect of ſpecifick gravity. And if the Pipe was thruſt deeper into the water, then the Bladder would be made protuberant inwards, as when the Pipe had water in it. By which it appears, that theſe *Phænomena*, without recourſe to *attraction*, may be explicated barely by the Laws of the *Æquilibrium* of Liquors.

FINIS.